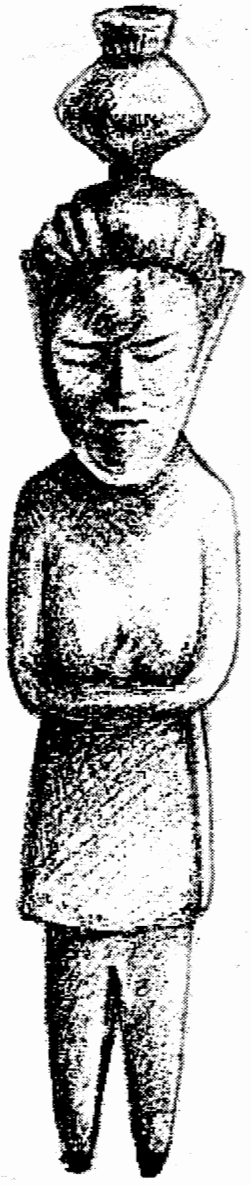
MOTHER TONGUE

NEWSLETTER OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY



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COMMENTS ON COLARUSSO'S PAPER "PHYLETIC LINKS BETWEEN PROTO-INDO-EUROPEAN AND PROTO-NORTHWEST CAUCASIAN"*

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INTRODUCTION

In the previous issue of *Mother Tongue*, John Colarusso presented evidence for a genetic relationship between Proto-Indo-European and Proto-Northwest Caucasian. He discussed the phonology of Proto-Indo-European and proposed a revised ("fortified") phonemic inventory for Proto-Indo-European, he listed several grammatical formants common to both language families, and he presented a number of lexical parallels, including preverbs, numerals, particles, and "conventional cognates."

He concluded that there was evidence, albeit preliminary, for a genetic relationship between Proto-Indo-European and Proto-Northwest Caucasian, and he posited a common proto-language, which he named "Proto-Pontic."

In my recently published co-authored book entitled *The Nostratic Macrofamily: A Study in Distant Linguistic Relationship* (Mouton de Gruyter [April 1994], 932 pp.), I present a considerable amount of evidence for a genetic relationship between Proto-Indo-European and certain other language families, to wit, Proto-Afroasiatic (Proto-Afrasian), Proto-Kartvelian ("South Caucasian"), Proto-Uralic-Yukaghir, Proto-Elamo-Dravidian, and Proto-Altaic (Mongolian, Turkic, Tungus, and probably Korean). Following Pedersen (and Illič-Svityč and Dolgopolsky), I posit a common ancestor named "Proto-Nostratic." I also list possible cognates found in Sumerian and note that Nivkh (Gilyak), Chukchi-Kamchatkan, and Eskimo-Aleut are probably to be included as members of the Nostratic macrofamily as well.

There is a growing feeling among many of the scholars studying Nostratic that Proto-Afroasiatic may be a sister language of Proto-Nostratic rather than a descendant language, while Proto-Indo-European is seen by Greenberg as being more closely related to Proto-Uralic-Yukaghir, Proto-Altaic, Nivkh, Chukchi-Kamchatkan, and Eskimo-Aleut, these forming a separate subgroup called "Eurasiatic."

However, Indo-European presents some special problems. On the one hand, its grammatical structure, at least in its earlier periods, more closely resembles those of its sister Eurasiatic languages; on the other hand, its phonological system more closely resembles the phonological systems found in Proto-Kartvelian and Proto-Afroasiatic, at least when using the revised Proto-Indo-European phonological system proposed by Gamkrelidze-Ivanov and Hopper. Moreover, there are typological problems with every phonological system proposed to date for Proto-Indo-European — one wonders, for example, why there are no affricates. This leads me to suspect that Proto-Indo-European may in fact be a blend of elements from two (or more?) different languages,[1] as has already been suggested by several other scholars. But a blend of what? In footnote 1 in his paper, Colarusso notes that "[t]he amateur archeologist Geoffrey Bibby suggested in 1961 that PIE was a Caucasian language that went north and blended with a Finno-Ugrian tongue." This suggestion merits closer consideration.

In this paper, I would like to discuss how Colarusso's theories shed possible light on these and other issues, noting both the strong points and limitations of his approach, and I will propose an alternative theory that I believe better fits the linguistic evidence.

PHONOLOGY

In Chapter 2 of my book, *The Nostratic Macrofamily*, I discuss the phonological systems of the various Nostratic daughter languages, establish sound correspondences, and posit a phonological system for the Nostratic proto-language, as follows:

1	2	3	4	5	6	7	8
p[h]	b	p'		m			w
t[h]	d	t'		n	r	l	
c[h]	ʒ	c'	s				
tʸ[h]	dʸ	t'ʸ	sʸ	nʸ	rʸ	lʸ	y
t͡ɬ[h]	-	t͡ɬ'					
kʸ[h]	gʸ	k'ʸ					
k[h]	g	k'		ŋ			
kʷ[h]	gʷ	k'ʷ					
q[h]	ɢ	q'					
-	-	q'ʷ					
			ħ	ʕ			
		ʔ	h				
	i ~ e		u ~ o				
		ə ~ a					
	iy ~ ey		uy ~ oy		əy ~ ay		
	iw ~ ew		uw ~ ow		əw ~ aw		

Colarusso (1994:18) sets up the following phonological system for Proto-Pontic:

1	2	3	4	5	6	7	8
pʰ	p	b	-		m		w
tʰ	t	d	t'		n	r	l
cʰ	c	ʒ	c'	s	z		
čʰ	č	ǯ	č'	š	ž		y
ƛʰ	ƛ	ƛ	ƛ'				
kʰ	k	g	k'	x̂	ĝ		
qʰ	q	-	q'	x	ɣ		
				ḫ	ʕ		

			ʔ		h		
i						u	
e		ə				o	
		a					

Though there are many points of agreement between Colarusso's Proto-Pontic and my Proto-Nostratic, the main differences are (A) that I do not posit a separate series of unaspirated voiceless obstruents (his column 2 above), (B) that he posits neither palatalized nor rounded tectals[2] nor palatalized alveolars, and (C) that I posit fewer laryngeals.

For Proto-Nostratic, I set up a series of non-phonemically aspirated voiceless obstruents (my column 1). There is actually some evidence, however, that two series may be warranted: (A) aspirated obstruents and (B) unaspirated obstruents — exactly what Colarusso has set up for Proto-Pontic. The evidence for this comes from Afroasiatic on the one hand and from Altaic on the other. For Proto-Afroasiatic, a separate phoneme **f* must be posited in addition to a voiceless bilabial stop **p*, and both of these correspond to voiceless bilabial stops in the other Nostratic daughter languages. Setting up two series at the Proto-Nostratic level makes it easy to account for Proto-Afroasiatic **f*, which would then be the reflex of an original phonemic voiceless aspirated bilabial stop **pʰ*. For Proto-Altaic reconstructions, I followed Nicholas Poppe and John Street, who posit a simple two-way contrast: (A) non-phonemically aspirated voiceless and (B) voiced obstruents. However, Illič-Svityč assumed that there was a three-way contrast in Proto-Altaic (I have not yet seen the recent book on Altaic by Starostin, *Altajskaja problema i proisxoždenie japonskogo jazyka [The Altaic Problem and the Origin of the Japanese Language]*, Moscow: Nauka [1991])[3]: (A) voiceless aspirated, (B) plain voiceless, and (C) voiced obstruents. (Initially, the earlier glottalics are assumed to have become deglottalized and to have merged with the plain voiceless obstruents in Altaic, while, medially, they are represented by voiced obstruents.) If Illič-Svityč's views are correct, Altaic may provide evidence for positing separate voiceless aspirated and voiceless unaspirated obstruents in Proto-Nostratic. Now, if it turns out that Proto-Northwest Caucasian is in fact related to Proto-Indo-European as Colarusso has tried to show, and, by implication, to Nostratic, additional (and clinching) evidence would be provided for setting up phonemic unaspirated beside phonemic aspirated obstruents at the Proto-Nostratic level.

Now, let us look at Proto-Indo-European itself. Colarusso sets up a three-way contrast for his "Fortified PIE": (A) voiceless aspirated, (B) plain voiced, and (C) glottalized. Gamkrelidze-Ivanov also set up a three-way contrast: (A) voiceless (aspirated), (B) voiced (aspirated), and (C) glottalized. In their system, the feature of aspiration is viewed as phonemically irrelevant, and the phonemes of (A) and (B) can be realized either with or without aspiration depending upon the paradigmatic alternation of root morphemes. They set up this alternation mainly to account for instances of Grassmann's Law. However, as pointed out by Brian Joseph in a paper read before the 1994 meeting of the Linguistic Society of America, this reconstruction runs into problems in Italic. Indeed, it will probably turn out that Grassmann's Law should not be viewed as pan-Indo-European but, rather, as operating strictly in certain dialect groups. Now, most scholars, regardless of whether they follow the traditional reconstruction of Proto-Indo-European or the radical revisions proposed by Gamkrelidze-Ivanov and Hopper, set up a three-way contrast for the obstruents — in other words, they do not set up phonemic unaspirated voiceless beside phonemic aspirated voiceless obstruents. The main exception is Oswald Szemerényi, who has argued consistently that two separate series should be set up. The fact is that in most instances the traditional voiceless aspirates can be explained as secondarily derived. Moreover, the evidence for their existence is restricted to one or two branches of Indo-European, and the examples found there are usually explained as developments specific to these branches. Nonetheless, there have always been a handful of examples that cannot be explained as secondarily derived. In light of Colarusso's proposals, the whole question may merit re-examination. It may turn out that Szemerényi has been right all along. Moreover, setting up phonemic aspirated voiceless beside unaspirated voiceless obstruents for Proto-Indo-European may eliminate some of the objections that have been raised against the revisions proposed by Gamkrelidze-Ivanov. Thus, one can envision a late (Post-Anatolian) Proto-Indo-European phonological system of the following shape:

1	2	3	4	5	6	7	8
p^h	p	b^h	(p')		m		w
t^h	t	d^h	t'	s	n	r	l
k^{yh}	k^y	g^{yh}	k'^y				y
k^h	k	g^h	k'				
k^{wh}	k^w	g^{wh}	k'^w				
				$\underset{\frown}{\text{ħh}}$	$\underset{\frown}{\text{ʕɦ}}$		
			ʔ	h			

At an earlier period (Pre-Anatolian), the voiced aspirates (column 3 above) may have been plain voiced, as I have argued elsewhere. It may be noted in passing that I view the Anatolian branch as the first to split off from the main speech community.

The palatalized and rounded tectals and palatalized alveolars that I reconstruct for Proto-Nostratic present special problems that have not yet been worked out to my satisfaction. Consequently, I will not comment further on these series in this paper. The same can be said for the affricates.

It seems to me that Colarusso posits a greater number of "laryngeal" phonemes for Proto-Indo-European than required either by internal Indo-European evidence or by evidence from the other Nostratic daughter languages. Extremely good and plentiful cognates containing "laryngeals" can be established between Proto-Indo-European and Proto-Afroasiatic, and the "laryngeals" are better preserved in the Afroasiatic branch than in any of the other Nostratic daughter languages. For Proto-Afroasiatic, only four "laryngeals" are posited by most scholars, though there is not unanimity here: (1) ʔ (glottal stop), (2) h (voiceless laryngeal fricative), (3) ħ

(voiceless pharyngeal fricative), and (4) ʕ (voiced pharyngeal fricative). There may have been rounded "laryngeals" in Proto-Afroasiatic as well. I would set up the same four "laryngeals" for Pre-Proto-Indo-European. I assume, however, that the voiceless and voiced pharyngeals became multiply-articulated pharyngeal/laryngeals in later Indo-European, and this is what I have shown in the reconstruction above. This assumption is made to account for their vowel-coloring properties. (I would like to note that this is an idea I originally got from Colarusso after reading his Ph.D. Dissertation [Harvard University, 1975].) The whole question concerning the "laryngeals" remains open, though. The quality and quantity of the cognates that can be established between Proto-Indo-European and Proto-Northwest Caucasian on the one hand and between Proto-Nostratic and Proto-Northwest Caucasian on the other, especially when the Afroasiatic evidence is brought into consideration, may require that additional "laryngeal" phonemes be set up for Proto-Nostratic, or it may show simply that additional "laryngeals" have arisen in Proto-Northwest Caucasian through developments specific to that language family.

NOMINAL SUFFIXES

Colarusso presents a series of nominal suffixes that he claims are common to Proto-Indo-European and Proto-Northwest Caucasian. For the most part, I find his examples to be well founded. I have reservations, however, about the following (the numbers refer to those given in his paper): in (16), I would not compare the PNWC suffix */-ĝa/ — it just does not seem to belong, both for phonological as well as semantic reasons; (17) should be removed completely — again, for phonological reasons; in (18), I see no reason for positing Proto-Pontic */-m/ when */-n/ is implied by both PIE and PNWC — I would take the */-m/ found in PNWC to be a separate ending, perhaps comparable to the */-m/ found in the PIE accusative case ending.

OTHER ENDINGS

Here, Colarusso considers "some other endings, such as participles, abstracts, cases, and such." All of the examples presented by Colarusso are convincing. The one comment I have is the same as I had above, namely, that I would distinguish between */-m/ and */-n/.

PREVERBS (OLD NOUNS) / PARTICLES

Colarusso lists three preverbs (old nouns) common to PIE and PNWC and also compares PIE "final *s" with PNWC old oblique in */-š/. All of these are convincing examples. Two ([34] and [35]) of the three preverbs have cognates in the other Nostratic languages.

Colarusso also lists two particles, both of which are convincing.

VERBAL DESINENCES AND SUFFIXES

Some of the parallels presented by Colarusso are intriguing and deserve further investigation. Specifically, I would like to see more about what PNWC might be able to tell us about the PIE athematic ~ thematic conjugational types.

I am skeptical about the PIE perfects discussed in (48). The PIE primary, active, present, athematic *-*i* discussed in (50) is usually derived from a deictic meaning "here and now"; the explanation offered by Colarusso presents a viable alternative, though this does not necessarily rule out ultimate derivation from a deictic.

The explanation given by Colarusso in (52) to PIE "*s*-movable" is not convincing and should be abandoned, and the same goes for the personal ending discussed in (53).

In (56), why the meaning "(in) hand"? I see nothing wrong with simply positing an "independent adverb before the verb denoting accomplishment of action" — this seems to be what is implied in both PIE and PNWC.

STEM FORMATION (à la Benveniste)

Colarusso actually presents an alternative explanation for certain stem patterns to that offered by Benveniste's theory of the Indo-European root. As he himself notes: "[f]urther work in this area promises to reveal some of the more obscure cognates between these two families as well as to throw light upon some of the more difficult laryngeal development within Indo-European history."

CONVENTIONAL COGNATES

Colarusso lists 20 possible cognates. I find the following to be the most convincing: (66) "sour, caustic liquid"; (70) "giant"; (75) "two"; (76) "six"; (79) "son, child, foster child"; (80) "son, nephew"; (81) "to sit (down)". PIE "fire" (64) and "period of time" (65) find more convincing explanations when compared with cognates in other Nostratic languages. But, note that Ubykh has /-fa-/ "to ignite" — is this really to be derived from PNWC */-pʰa-/ "down; to descend", or is it a separate stem in its own right? In other words, if there were justification for setting up a distinct PNWC */-pʰa-/ "to ignite", then comparison with the PIE might be possible and permit us to posit a Proto-Pontic */pʰa-x̂ʷə-r/ "fire", which we could then easily compare with forms in other Nostratic languages. (76) "six" and (80) "son, nephew" also have cognates in other Nostratic languages.

ADDITIONAL COGNATES

I would like to propose some additional cognates. I have taken the following Northwest Caucasian examples exclusively from Kuipers's *A Dictionary of Proto-Circassian Roots* — it is the only work available to me. Now, I realize full well that Circassian is but one branch of Northwest Caucasian. Therefore, adjustments may have to be made to the cognates I am proposing on the basis of evidence from the remaining branches of Northwest Caucasian. Moreover, I have brought in

evidence from other Nostratic languages. Proto-Indo-European reconstructions are in accordance with the revisions proposed by Gamkrelidze-Ivanov and Hopper.

The following abbreviations will be used: PCirc. = Proto-Circassian; PN = Proto-Nostratic; PIE = Proto-Indo-European; PK = Proto-Kartvelian; PAA = Proto-Afroasiatic; PEC = Proto-East Cushitic; PHEC = Proto-Highland East Cushitic; PSC = Proto-Southern Cushitic; PU = Proto-Uralic; PFU = Proto-Finno-Ugrian; PD = Proto-Dravidian; PAlt. = Proto-Altaic; CT = Common Turkic; S = Sumerian. The "P" will be omitted when no proto-form is posited but when there are cognates from that branch of Nostratic. Other language names will be spelled out in full.

1. PCirc. *pʰərx̂ᵒa "passageway, porch" ~ PN *p[ʰ]ar-/*p[ʰ]ər- "to go or pass; to go or pass over or across; to go forth or out" > PIE *p[ʰ]er-/*p[ʰ]or-/*p[ʰ]r̥- "to go or pass; to go or pass over or across", (n.) *p[ʰ]r̥-t[ʰ]- "passage, crossing, way, ford" (cf. Latin porta "gate, entrance", porticus "a portico, collonnade, arcade, gallery"); AA: Egyptian prỉ "to go, to come out, to go forth, to go up, to ascend", prw "motion, procession, outcome, result", prt "(ritual) procession"; S pàr "to go or pass by, to go past".

2. PCirc. *bayə "rich" ~ PN *bay-/*bəy- "to apportion, to divide into shares, to distribute, to allot" > PIE *b[ʰ]ey-/*b[ʰ]oy- "to give" (found only in Anatolian); PAA *bay-/*bəy- "to apportion, to separate into equal shares, to distribute into shares"; PAlt. *bāya(n) "rich"; S ba "to give as a gift or a ration".

3. PCirc. *mə "this", *maw "thither, that", *.../ma "there you are!" ~ PN *ma-/*mə- (also *mu-/*mo-) demonstrative stem > PIE *mo- (found vestigially in Celtic); PK *ma- "this, he"; PFU *mu "other, another"; Alt.: CT (*mū/*mō >) (nom. sg.) *bū/*bō, (oblique) *mu-n-; Mongolian mön deictic word serving as a demonstrative pronoun, adjective, adverb, and copula.

4. PCirc. *mə negative prefix ~ PN *ma(ʔ)/*mə(ʔ) negative/prohibitive particle > PIE *mē prohibitive particle; K: Svan (particle of modal negation) mād "no, not", mām(a) "not", māma "no"; PAA *ma(ʔ) negative/prohibitive particle.

5. PCirc. *mana "penis" ~ PN *manʸ-/*mənʸ- "to lust after, to desire passionately, to copulate, to have sexual intercourse, to beget" > (?) IE: Modern Irish mian "desire", Welsh mwyn "enjoyment, value; gentle, kind, dear"; PAA *man-/*mən- "to lust after, to desire passionately, to copulate, to have sexual intercourse, to beget"; PD *maṇ- "to love, to wed, to copulate with; to be united with, to marry", *māṇi "penis". PN *manʸ-/*mənʸ- "progenitor, begetter, man, male" (derivative of the preceding) > PIE *man(u)- "man, begetter, progenitor"; (?) AA: PHEC *man(n)- "man, person; (pl.) people", PEC (with fossilized feminine suffix) *man-t-/*min-t- "woman" (note also Bayso man-to "penis", man-tiiti "vagina", Burji múnn-aa "vagina"); PFU *manʸtʸɜ "man, male"; D: (?) Tamil māntar "human beings, male persons", Naikṛi más "man, husband", Parji mañji "man".

6. PCirc. *nag(a) "bad, evil; to disfigure" ~ PN *nag-/*nəg- "to strike, to split, to pierce" > PIE *neg[ʰ]-/*nog[ʰ]- "to strike, to split, to pierce"; PAA *nag-/*nəg- "to strike, to split, to pierce".

7. PCirc. *napʰa "face" ~ PN *nap[ʰ]-/*nəp[ʰ]- "to breathe, to blow" > PIE *np[ʰ]- > (with metathesis) *p[ʰ]n-ew-/*p[ʰ]n-ew-/*p[ʰ]n-u-, *p[ʰ]n-ek[ʰ]- "to breathe, to blow"; PAA *naf-/*nəf- "to breathe, to blow" (cf. Hebrew 'aφ "nose, nostril, face", Arabic 'anf "nose, forepart of anything").

8. PCirc. *k'ak'a "to chirp" ~ PN *k'ak'- "to cackle, to chatter" > PIE *k'ak'- "to cackle, to chatter"; PK *k'ak'a-n- "to cackle"; PAA *k'ak'- "to cackle, to make a noise"; PD *kak- "to laugh".

9. PCirc. *q'ᵒatʰa "to tell, to report; to announce, to make known" ~ (?) PN *k'ʷatʸ[ʰ]-/*k'ʷətʸ[ʰ]- "to say, to speak, to call" > PIE *k'ʷet[ʰ]-/*k'ʷot[ʰ]- "to say, to speak, to call"; PFU *kutʸɜ- "to call, to summon".

10. PCirc. *q:ara "black" ~ PN *k[ʰ]ar-/*k[ʰ]ər- "black, dark" > PIE *k[ʰ]er-s-/*k[ʰ]r̥-s- "black, dark"; PD *kaṛ- "black, dark; to grow black, to darken"; PAlt. *kara "black".

11. PCirc. *bənə "(children of) family" (semantic development as in Mehri and Hebrew cited below) ~ PN *banʸ-/*bənʸ- "to join together, to fit together, to fasten, to twist together, to form or produce in any way" > PIE *b[ʰ]end[ʰ]-/*b[ʰ]ond[ʰ]-/*b[ʰ]n̥d[ʰ]- "to join together, to fit together, to fasten, to twist together, to form or produce in any way"; PAA *ban-/*bən- "to join together, to fit together, to fasten, to twist together, to form or produce in any way" (cf. Mehri ḥə-bōn "children"; Hebrew bēn "son, grandson; (pl.) children [both sons and daughters]"); PD *paṇ- "to make, to do, to produce, to build".

12. PCirc. *k'ᵒad(a) "to disappear, to get lost, to perish" ~ PN *k'ʷad-/*k'ʷəd- "to strike, to wound, to hurt, to slay" > PIE *k'ʷed[ʰ]-/*k'ʷod[ʰ]- "to strike, to wound, to hurt, to slay" (cf. Lithuanian gendù, gésti "to go out, to die out, to become dim"); K: Georgian k'vd- "to die"; PD *kuṭṭ- "to strike, to knock, to pound".

13. PCirc. *t'awə "to bump (one's head)" ~ PN *t'aw-/*t'əw-

"to hit, to strike" > PIE *t'ew-/*t'ow-/*t'u- "to hit, to strike"; PAA *t'aw-/*t'əw- "to hit, to strike"; S du_7 "to butt, to gore".

14. PCirc. *wasa "price" ~ PN *wus-/*wos- "to trade, to deal" > PIE *wes-/*wos- "to trade, to deal", (n.) *wes-no-m "price" (cf. Sanskrit vasná-m "price, value"); PFU *wosa "trade, commerce".

15. PCirc. *λaɬa "rivulet" ~ PN *laɬ-/*ləɬ- "to make flow, to pour, to moisten, to wet" > PIE *leɬ̲h- [*laɬ̲h-] (extended form *leɬ̲h-w/u- [*laɬ̲h-w/u-]) "to pour, to pour out (liquids)", (n.) *leɬ̲h-no-s [*laɬ̲h-no-s] (> *lā-no-s), *leɬ̲h-mo-s [*laɬ̲h-mo-s] (> *lā-mo-s) "anything that contains water or liquid: puddle, trough, tub, vat, etc." (cf. Greek ληνός "anything shaped like a tub or trough: a wine-vat, a trough [for watering cattle], a watering-place", λήμη "rheum"; Latin lāma "marsh, puddle"; Old Icelandic lón "lagoon, inlet"; Latvian lāns "puddle", lāma "puddle"); PAA *laɬ-/*ləɬ- "to make flow, to pour, to moisten, to wet"; S làh "to wash, to clean; (n.) laundry, wash".

16. PCirc. *gal(a) "to slip / to (slip and) fall" ~ (?) PN *g^{y}il-/*g^{y}el- "to glide, to slip, to slide" > PIE *g[ʰ]ley-/*g[ʰ]loy-/*g[ʰ]li- "to glide, to slip, to slide"; PAA *g^{y}al-/*g^{y}əl- "to glide, to slip, to slide"; PFU *kilɜ (*külɜ) "smooth, slippery".

17. PCirc. *warda "high-born", *warq:ə "nobleman" ~ PN *war-/*wər- "to raise, to elevate; to grow, to increase; (n.) uppermost, highest, or topmost part" > PIE *wer-d[ʰ]-/*wor-d[ʰ]-/*w̥r̥-d[ʰ]- "to raise, to elevate; to grow, to increase", (adj.) *word[ʰ]-o-s "grown, full-grown, tall, upright", *w̥r̥d[ʰ]- and *w̥r̥Hd[ʰ]- (> *w̥r̥̄d[ʰ]-) "raised, upright, tall", *wers-/*wors-/*w̥r̥s- and *werk[ʰ]s-/*work[ʰ]s-/*w̥r̥k[ʰ]s- "uppermost, highest, or topmost part"; AA: Egyptian wr "great, important; much, many; eldest", wrr "to be great, to make great, to increase, to grow large", wr "greatness; great one, chief"; PFU *wärä "(wooded) hill or mountain"; PD *varay "mountain, peak", *vār "length, elongation; height, straightness".

18. PCirc. *waλa "to totter, to undulate" ~ PN *wal^{y}-/wəl^{y}- "to turn, to roll, to revolve" > PIE *wel-/*wol-/*w̥l̥- "to turn, to roll, to revolve"; PAA *wal-/*wəl- "to revolve"; PD *val̥- "to surround, to walk around, to walk in a circle".

19. PCirc. *wəlaşa "to pound, to crush", *wəlawə "to get tired" ~ PN *wal-/*wəl- "to crush, to grind, to wear out; to be worn out, weak; to fade, to wither, to waste away" > PIE *wel-/*wol-/*w̥l̥- "to crush, to grind, to wear out; to be worn out, weak; to fade, to wither, to waste away"; PD *val- "to be tired; to become emaciated, thin; to ache", *ol- "to grow weak or faint; to become reduced, slender, thin, emaciated"; PAlt. *öl- "to be weak from hunger, to wither, to fade, to starve to death". PN *wal-/*wəl- "to strike, to wound, to destroy" (probably identical to the preceding) > PIE *wel-/*wol-/*w̥l̥- "to strike, to wound"; PD *vel- "to conquer, to overcome, to subdue, to destroy".

20. PCirc. *k'ana "piece, lump" ~ (?) PN *k'^{y}un-/*k'^{y}on- "to bend or fold together, to crack, to split, to divide" > PIE (*k'en-/*k'on-/)*k'n- "to bend or fold together, to crack, to split, to divide", (n.) *k'onk'o-s "lump", *k'onk'ulo-s "lump, ball, bundle", *k'noHt'o-s (> *k'nōt'o-s) "lump, knot", *k'nut[ʰ]o-s "lump, knot"; PK *k'on- "to tie or bind together" (cf. Georgian k'on-a "bundle, bunch"); PAA *k'^{y}an-/*k'^{y}ən- "to bend or fold together, to crack, to split, to divide".

21. PCirc. *t:aλa "to splash, to threaten; to shake (fist), to wave threateningly; to rattle (saber)" ~ PN *t[ʰ]al^{y}-/*t[ʰ]əl^{y}- "to push, to thrust" > PIE *t[ʰ]el- "to push, to shove, to strike, to hit" (Pokorny 1959:1062 *teleĝh- "to hit" [?], *telek- "to push, to shove, to hit"); PAA *t[ʰ]al-/*t[ʰ]əl- "to push, to thrust"; PFU *tol^{y}ɜ- "to push, to shove, to thrust"; PD *taḷ- "to push, to shove, to thrust, to press through".

22. PCirc. *bəλə "to hide" (< "to spread out, to cover over") ~ PN *bul-/*bol- "to swell, to spread out, to overflow, to puff up, to inflate" > PIE *b[ʰ]el-/*b[ʰ]ol-/*b[ʰ]l̥- "to swell, to puff up, to inflate, to expand, to bubble up, to overflow"; K: Georgian *blom- "multitude" in (adv.) blomad "in a crowd, mass, mob, multitude"; PAA *bal-/*bəl- "to swell, to expand, to spread out, to overflow" (cf. PEC *balba[a]l- ["to spread out, to cover" >] "curtain, canopy, covering, etc."); S bul "to blow, to breathe, to puff".

23. PCirc. *t'əλə "to lie down" ~ PN *t'al-/t'əl- "to stretch, to extend" > PIE (*t'el-/*t'ol-/*t'l̥- "to stretch, to extend, to lengthen":) *t'l̥-H-g[ʰ]- "long", *t'l-eE-g[ʰ]- (> *t'l-ē-g[ʰ]-) "to stretch, to extend, to lengthen"; PAA *t'al-/t'əl- "to stretch, to extend"; S dalla "to widen, to stretch, to extend, to enlarge". For the semantics, cf. Old Church Slavic kladǫ, klasti "to lie down; to lay, to put" vs. Lithuanian klóju, klóti "to lay out, to spread out".

24. PCirc. *q:ərə "stony land, rock" ~ PN *k[ʰ]ar- "hard, strong, firm" > PIE *k[ʰ]ar- "hard, strong, firm" (cf. Sanskrit karkara- "hard, firm", also "stone, limestone"); PAA *k[ʰ]ar- "hard, dry"; PD *karu "strength, power".

25. PCirc. *mark'^{o}a "mulberry, blackberry" ~ PN *mur-/*mor- "mulberry, blackberry" > PIE *mor- "mulberry, blackberry"; AA: Egyptian mr "mulberry-tree"; PU

*mura "Rubus chamaemorus" (note also PFU *marya "berry").

26. PCirc. *hat:a "father" ~ PN *ʔat[ʰ](t[ʰ])-/*ʔət[ʰ](t[ʰ])- "father" > PIE *ʔat[ʰ](t[ʰ])- "father"; AA: Egyptian ỉt "father"; PED *atta "father"; PA *etiké(y) "older male relative".

27. PCirc. *hana "mother" ~ PN *ʔanʸ- "mother, aunt" > PIE *ʔano-s "mother"; AA: PSC *ʔaŋ- "father's sister"; PU *anʸa "mother, aunt"; PD *aṇṇ- "mother, a woman, father's sister".

28. PCirc. *λa "leg, foot" (derivatives include: *λakʰə "strong, firm, staunch [of person]" [contains *λa "leg" and an unclear element]; *λaq:ºa "leg"; *λasə "on foot, unmounted"; *λambə "footprint"; *λačʰ'a "lame"; *λagºə "footsole, floor, valley") ~ PN *lak[ʰ]-/*lək[ʰ]- "leg, foot" > PIE *lak[ʰ]- "leg, foot"; PAA *lak[ʰ]-/*lək[ʰ]- "leg, foot".

29. PCirc. *n(a) "mother" (derivative: *n[a]wa "old woman, granny") ~ (?) PN *nat'- "woman, female relative" > AA: PSC *nat'a "woman"; PU *nata "sister-in-law, younger brother of the husband or the wife"; PD *nāt- "husband's sister, sister-in-law".

30. PCirc. *nartʰa "Nart" (mythical hero) ~ PN *nir-/*ner- "to be strong, manly, virile" > PIE *ner- "to be strong, manly, virile; (n.) man, hero", *ner-t[ʰ]o-s "manly; manliness, virture, strength", *ner-yo-s "manly; man"; AA: Egyptian nr "to be strong, mighty", nrw "power, strength, victory, valor, mighty one"; S ner "prince".

31. PCirc. *wala "cloud" ~ PN *wal-/*wəl- "to flow, to wet, to moisten" > PIE *welk[ʰ]-/*wolk[ʰ]-, *welk'-/*wolk'-/*w̥lk'-, *welg[ʰ]-/*wolg[ʰ]-/*w̥lg[ʰ]- "to wet, to moisten", (adj.) *wolg[ʰ]o-s "lukewarm, damp, moist" (cf. Old English weolcen, wolcen "cloud", German Wolke "cloud"); AA: Arabic waliḫa-t "well-watered, rich in vegetation"; PD *ol- "to flow", *vāl- "to drizzle, to drip".

32. PCirc. *wašʰ'ə "ax" (cf. *w[a] "to beat, to strike, to shoot, to sting, to attack; to smooth [a threshing floor]"; derivatives include: *wəb[a] "to trample, to pound, to crush; to slander"; *wəp'a "to cut open [an animal], to skin"; *wətʰa "to trample, to knead clay with the feet"; *wət:[a] "to pound, to thresh [sunflower seeds, maize], to beat [egg], to knead, to break off"; *wək'ə "to kill"; *wat:a "hammer") ~ PN *wasʸ-/*wəsʸ- "to beat, to strike, to crush, to grind, to pound, to wear out; to be or become worn out, tired, weary, fatigued, exhausted" > PIE *wes- "to crush, to grind, to pound, to wear out; to wither, to fade, to rot, to waste away"; AA: Egyptian wšš "to crush, to pound", wšm "to slay, to crush, to chop up, to split, to pound together"; PFP *wäsʸä "to be or become tired, weary, fatigued, exhausted"; PD *vēc- "to grow tired, fatigued, weary", *vāc- "to cut off, to chip off, to smooth by chipping, to shave down", *vācci "adze, scraper". Buck (1949:561-562, §9.25 ax) notes that words for "ax" are typically derived from various roots for "cut" or "strike", and this is the derivation that I assume for both PCirc. *wašʰ'ə "ax" and PD *vācci "adze, scraper".

33. PCirc. *k:atʰa "sword" ~ PN *k[ʰ]at[ʰ]-/*k[ʰ]ət[ʰ]- "to beat, to strike, to fight" > PIE *k[ʰ]at[ʰ]- "to fight"; AA: Egyptian ktkt "to beat, to strike"; PD *kat- "to be angry with; (n.) anger, wrath", *kāt- "to kill, to murder; to cut, to divide; to quarrel, to fight; (n.) fight, battle, war", *katti "knife, sword, razor", *katk- "to strike down, to cut with an ax".

In the following additional cognates, only Proto-Circassian and Proto-Indo-European forms are given — I have been unable to locate cognates in other Nostratic languages.

34. PCirc. *nəba "belly" ~ PIE (*neb[ʰ]-/)*nob[ʰ]- "navel" (Pokorny 1959:314-15 reconstructs [*enebh-], *embh-, *ombh-, *nŏbh-, [*nēbh- ?], *m̥bh-). Note here also Temirgoy nəbəǯ'ə/bənža "navel", Ubykh nəbəǯ' "navel".

35. PCirc. *ban(a) "to fight" ~ PIE *b[ʰ]en- "to slay, to wound".

36. PCirc. *malə "sheep" ~ PIE *mel- "wool, woollen garment".

37. PCirc. *hawa "but" ~ PIE *hew- [*haw-] "that, other" (cf. Gothic auk "but, also"; Latin au-tem "but, on the other hand").

38. PCirc. *kʰakʰa "to chirr, to laugh derisively, to bleat, to howl, to shout" ~ PIE *k[ʰ]ak[ʰ]a exclamation indicating laughter (onomat.).

39. PCirc. *p:əyə "enemy" ~ PIE *p[ʰ]ĕ(y/i)- "to hurt, to harm, to attack" (Pokorny 1959:792-93 reconstructs *pē[i]-; note also Mann 1984-1987:913 *pē i̯ō "to hurt, to harm, to attack, to blame" and 935 *pī i̯ō "to hate, to revile") (cf. Gothic fijands "enemy", Old English fēond "enemy").

40. PCirc. *yatʰa "to rage (storm); to become insolent" ~ PIE *yet[ʰ]- "to let fly at something" (Mann 1984-87:444 reconstructs PIE *i̯etō "to dash, to dart, to proceed, to strive", *i̯etis "motion, eagerness, dash, passion").

41. PCirc. *k'əyə "to shreik, to howl" ~ PIE *k'ĕ(y/i)- "to sing, to call, to cry out" (Pokorny 1959:355 reconstructs *gē[i]-: *gō[i]-: *gī-).

42. PCirc. *k'ərə "thick, dense (of wool, beard, etc.), long (of hair), high (of grass)" (cf. *k'[a] "to come out, to bud, to grow") ~ PIE *k'ar-k'əro-s "mass, pile, heap" (Mann 1984-1987:265 reconstructs *gargəros, -i̯os).

43. PCirc. *k'anə "knucklebone (used in bone game)" ~ (?) PIE *k'enu "knee".

44. PCirc. *k'ºasa "to go out (as fire, light); to escape, to run away, to desert, to elope" ~ PIE *k'ʷes- "to extinguish" (cf. Lithuanian gèsti "to go out, to die out, to become dim").

45. PCirc. *q'ºayə "to bleat" ~ (?) PIE *k'ʷey- "to complain, to bemoan" (Pokorny 1959:567 reconstructs *gᵘei-).

46. PCirc. *warə "wave; turbulent" ~ PIE *wer-/*wor-/*wr̥- "to twist, to turn".

47. PCirc. *sama "heap" (< "to gather together, to pile up, to heap up") ~ PIE *sem-/*som- ("to gather together" >) "together, together with; one" (cf. Sanskrit sa [< *sm̥-] "with, together with, along with", sám "with, together with, along with, together, altogether", sa-trā́ "together, together with", sámana-ḥ "meeting, assembly; amorous union, embrace", samūbhá-ḥ "heap, collection").

48. PCirc. *kʰamə "to be insufficient, to lack" ~ PIE *k[ʰ]em- "lacking horns, hornless".

49. PCirc. *gəya "smooth (of ice)" ~ PIE *g[ʰ]ey- "snow, ice, winter" (cf. Sanskrit himá-ḥ "snow, frost, hoar-frost, winter", hemantá-ḥ "winter, the cold season"; Greek χιών "snow; snow-water, ice-cold water", χεῖμα "winter-weather, cold, frost", χειμών "winter; wintry weather, a winter storm"). The Northwest Caucasian word may be a loan from Indo-European.

50. PCirc. *q:ºətʰa "to smash, to break, to chop" ~ (?) PIE *kʷ[ʰ]et[ʰ]- in *kʷ[ʰ]et[ʰ]-wer- "four", assuming semantic development as follows: *kʷ[ʰ]et[ʰ]- "to cut into (equal) parts", *kʷ[ʰ]et[ʰ]-wer- "having equal sides" > "square, four-sided" > "four" (cf. Sanskrit catvará-m "square, cross-roads", thematic extension of a neuter noun *cátvar, which has not been preserved; also Latin -quetrus in triquetrus "three-cornered, triangular" and quadra [with -dr- from -tr-] "a square"). Though this etymology is highly speculative, it is not impossible. Clearly, the word for "four" in PIE is a derivative and is probably a fairly late creation. Note that the Anatolian languages have quite a different word for "four": Hittite meu-, Cuneiform Luwian ma-a-u-wa-, Hieroglyphic Luwian mawa-, Lycian *mu- in mupm̃m- "four-fold" (Lycian also has kadr-[ñna] "four" [this is not listed in Melchert's *Lycian Lexicon*]) — these may reflect the original PIE word for "four".

THE MAKE-UP OF INDO-EUROPEAN

Introductory Remarks

As noted in the Introduction to this paper, the hypothetical Indo-European parent language presents some special problems. To repeat, Proto-Indo-European shares a number of lexical and grammatical cognates with certain other language families of northern, central, and eastern Eurasia, especially Uralic-Yukaghir, and this was the reason that Greenberg included Indo-European as a member of his proposed Eurasiatic language family. When one looks at the phonology of Proto-Indo-European, however, especially when the revisions proposed by Gamkrelidze-Ivanov and Hopper are brought in, one is reminded of a Caucasian language — but certainly not a typical Caucasian language.

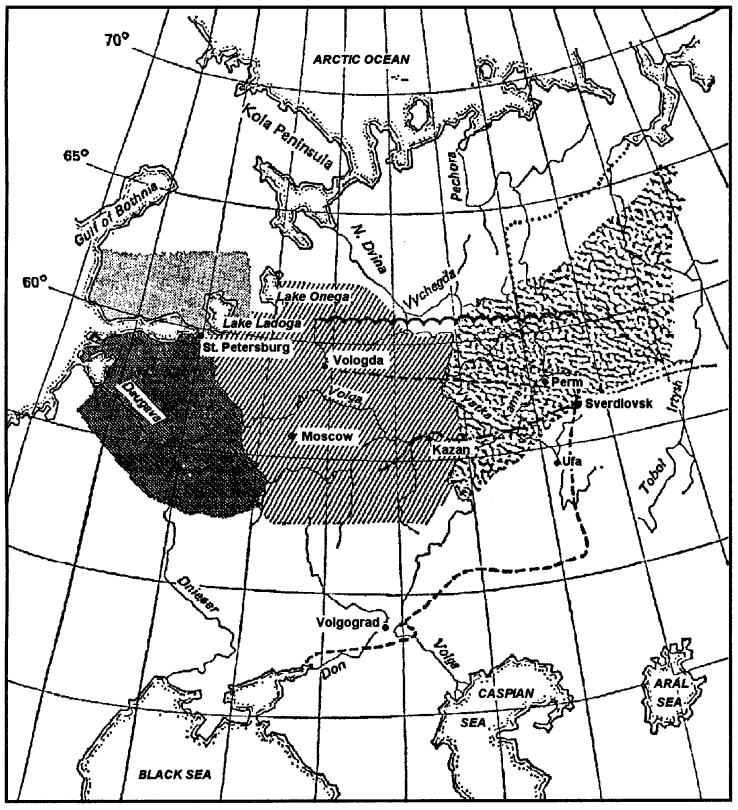
Typically, the phonological systems of the indigenous languages of the Caucasus Mountains region are characterized by extremely large inventories of consonantal phonemes. For example, the consonantal system of Ubykh contains 80 consonantal phonemes (or 83, when one considers the velar triad found in words of foreign origin [cf. Comrie 1981:202]). In contrast, the Proto-Indo-European phonological system is a model of simplicity. (The phonology of Northwest Caucasian is discussed in great detail by Colarusso in his 1975 Ph.D. dissertation *The Northwest Caucasian Languages: A Phonological Survey*.)

When one looks for simplicity, nothing could be more straightforward than the phonological system traditionally posited for Proto-Uralic, which is usually given as follows:

1	2	3	4	5	6
p	t		č	ć	k
	δ			δ′	γ
	s		š	ś	
m	n			ń	ŋ
	r	l		l′	
w				j	

In my co-authored book, *The Nostratic Macrofamily*, I use a slightly different notation for the phonemes in column 5: t^y (= ć), $δ^y$ (= δ′), s^y (= ś), n^y (= ń), l^y (= l′), y (= j) — this is an adaptation of the transcription proposed by Gyula Décsy. An additional sibilant, namely *š́, is sometimes also posited. It may be noted in passing that I take the phonemes traditionally transcribed as *δ and *δ′ to have been *t͡ɬ (or simply *ɬ) and *t͡ɬ^y (or simply *ɬ^y — a rare phoneme typologically, but found, for example, in Kabardian) respectively.

The reconstruction of the Proto-Uralic vowels, however, presents many problems, and there is still no consensus.



Uralic-Yukaghir

The primeval homeland of the Finno-Ugrians around 3,000 BCE is shown in the above map (this is taken from Hajdú 1975:31). What is important here is that, in terms of both dating and geography, the Finno-Ugrians were contiguous with Indo-Europeans, who were located just to the south in the area north of the Black Sea and extending eastward well beyond the Caspian Sea, that is, along the whole southern periphery of the Finno-Ugrian speech area. At this late date, however, the Indo-European proto-language had already begun to split up into different dialect groups. 3,000 BCE was the period of contact between Finno-Ugrians and the Indo-European dialect group that was developing into Indo-Iranian. The undifferentiated Indo-European parent language is usually placed at about 4,000 BCE or perhaps a little earlier.

The date at which the unified Uralic proto-language is thought to have been spoken is usually given as approximately 4,000 BCE, which would make it roughly contemporaneous with the period of Indo-European unity, while bringing in Yukaghir pushes this date back another millennium or so. Pushing the date backward in time also narrows the range of the speech community and moves its location further to the east. Décsy (1990:9), for example, places the Uralic proto-language "in the Forest Zone-Steppe-Border (mainly north of it) between the Volga Bend in Eastern Russia and the Ob River in Western Siberia."

Now, during the Würm glaciation, which peaked about 20,000-18,000 years ago, most of northern Eurasia was covered by huge sheets of ice, while treeless steppe-tundra stretched all the way from the European shores of the Atlantic Ocean in the west to well beyond the Ural Mountains in the east. It was not until about 15,000 years ago that the ice sheets began to recede in earnest. Clearly, most of what is considered to have been the Uralic(-Yukaghir) homeland was inhospitable to human habitation at one time. Therefore, I assume that, at a much earlier time, Pre-Uralic speaking people were located even further to the east and south and that they moved slowly westward and northward exploiting the opportunities created by the receding ice sheets.

Indo-European and Uralic-Yukaghir

Both Indo-European and Uralic-Yukaghir are usually considered to be members of the Nostratic macrofamily (so, for example, Illič-Svityč 1971- , Dolgopolsky 1984, and Bomhard-Kerns 1994), while Greenberg (forthcoming) maintains that Indo-European is to be included as a member of the Eurasiatic language family along with Uralic-Yukaghir, Altaic, Nivkh (Gilyak), Chukchi-Kamchatkan, and Eskimo-Aleut. In my co-authored book *The Nostratic Macro-family*, I accept Greenberg's classification and set up a distinct Eurasiatic subgroup within Nostratic.

The following evidence provides the basis for considering Indo-European to be genetically related to the other Eurasiatic languages, especially Uralic-Yukaghir: (A) Cognates in the system of pronoun stems (cf. Bomhard-Kerns 1994:3-7, Dolgopolsky 1984, Greenberg forthcoming, and, especially, Kerns 1985:11-51) — after carefully comparing and analyzing the pronoun stems of Indo-European, Uralic, and Altaic, Kerns (1985:48) states unequivocally: "The results are overwhelming. We are forced to conclude that the pronominal agreements between Indo-European and Uralic, between Uralic and Altaic, and between Indo-European and Altaic, did not develop independently, but instead were CAUSED by some UNIQUE historical circumstance. In short, it is extremely unlikely that the three pronominal systems could have evolved independently." (B) Morphological cognates (cf. Bomhard 1988 for a comparison of Indo-European athematic verbal endings with Uralic verbal endings, Collinder 1934 for Indo-European and Uralic, and Greenberg forthcoming for all of Eurasiatic). (C) Lexical cognates (cf. Bomhard-Kerns 1994, Illič-Svityč 1971- , Joki 1973, and Greenberg forthcoming).

Taken together, the evidence for a genetic relationship between Indo-European and the other Eurasiatic languages is much stronger by far than the evidence presented by Colarusso for a genetic relationship between Indo-European and Northwest Caucasian. Moreover, Indo-European and Uralic-Yukaghir appear to be particularly close and may represent a separate subbranch within Eurasiatic. For the sake of argument, I will assume this to have been the case — this subbranch may be called simply "Indo-Uralic".

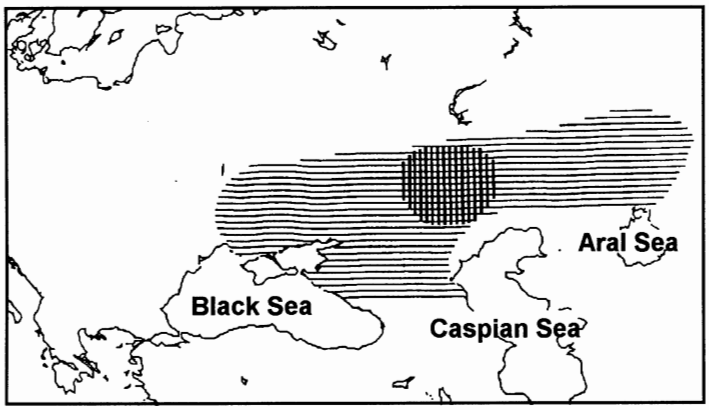
I have proposed (Bomhard-Kerns 1994:35) that Proto-Eurasiatic be dated to about 7,000 or 8,000 BCE, though a little earlier is also possible. I would place Proto-Indo-Uralic to about the same time period, that is, 7,000 BCE, and I would locate Proto-Indo-Uralic in Central Asia, as Nichols (1993) has done for what she calls "Pre-Indo-European". I believe that Nichols (1993) is correct in seeing a westward spread of Pre-Indo-European across the steppes, though I would use the term

"Indo-Uralic" instead for the initial period of westward movement. However, as Indo-Uralic continued its westward movement, I assume that it split into main two dialect groups, with what was to become Uralic-Yukaghir following a more northerly route and with what was to become Indo-European following a more central route straight across the steppes. It is this southern dialect that may be called "Pre-Indo-European".

One final point may be made here: I believe that the Proto-Indo-Uralic phonological system had a voicing contrast in obstruents, and this was preserved in Pre- and Proto-Indo-European. It was only in Proto-Uralic-Yukaghir that the voicing contrast was eliminated.

Indo-European and Caucasian

The westward spread of Pre-Indo-European eventually brought it to the shores of the Black Sea at 5,000 BCE or thereabouts. Though it is not known what language or languages were spoken in this area before the arrival of the Pre-Indo-European-speaking people, it is known that the Pre-Indo-Europeans were not the first inhabitants of the area. I can envision at least three possibilities: (A) the language or languages were related to those of the Caucasus Mountains; (B) Pre-Uralic-Yukaghir-speaking people may have arrived in the area first and have been there when the Pre-Indo-Europeans arrived; (C) one or more unknown languages were spoken there. Scenario (B) is highly unlikely on both linguistic and archeological grounds. That leaves us with scenarios (A) and (C) as viable alternatives.

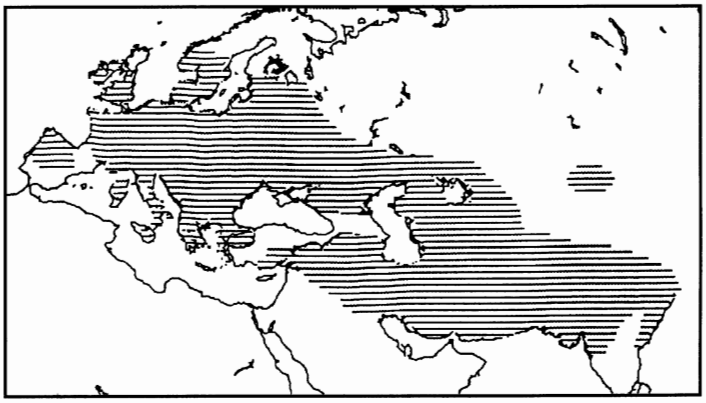


According to Villar (1991:15), the above map shows the homeland of Indo-European-speaking people at about 5,000 - 4,500 BCE, while the stroked area above the Caspian Sea indicates the earliest probable location of the Indo-Europeans.

We can now return to the suggestion made by Colarusso (1994:8) that "PIE and PNWC are genetically related at a phyletic level". In view of the strong evidence for a genetic relationship between Proto-Indo-European and other Eurasiatic (and Nostratic) languages, Colarusso's proposal seems unlikely. However, what Colarusso has done is that he has rightly identified Caucasian elements in Indo-European.

I will venture a guess that, when the Pre-Indo-Europeans arrived on the shores of the Black Sea, they met and mingled with Caucasian-speaking people (scenario [A] above). It was this contact that eventually produced Proto-Indo-European, with its greatly simplified Caucasian-looking phonological system and with its grammatical structure and system of pronominal stems bearing strong resemblances to what is found in Uralic. While maintaining contact with what was to become Uralic on the one hand and with the languages of the Caucasus on the other, Indo-European gradually assumed its own identity distinct from both Uralic and Caucasian.

Scenario (C) above is also a possibility, in which case Indo-European would have replaced whatever language or languages were spoken in the area. This means that there is practically no way to determine what that language or those languages were, except, perhaps, through the analysis of the Indo-European lexicon to try to find possible early loans. In this scenario, the contact between Indo-European and Caucasian languages could be accounted for by assuming that there was a northward migration of Caucasian-speaking people into Indo-European-speaking territory, somewhat along the lines that Colarusso has suggested. In my opinion, scenario (C) is less attractive than scenario (A).



The above map shows the area to which Indo-European languages had spread by the first century BCE (this is taken from Villar 1991:17).

CONCLUSION

Thus, Proto-Indo-European is not to be seen as a Caucasian language but rather as a caucasianized form of the Indo-Uralic (more accurately, Pre-Indo-European) dialect that had spread to the shores of the Black Sea. On the basis of grammatical, pronominal, and lexical cognates with other Eurasiatic languages, Proto-Indo-European is indeed to be classified as a Eurasiatic language, exactly as Greenberg has done. However, due to long-term contact with and extensive borrowing from Caucasian languages, Proto-Indo-European developed many characteristics which set it apart from the other branches of Eurasiatic and gave it a Caucasian-like appearance. In other words, we are talking about an early Pontic-Caucasus Sprachbund area which included languages belonging to more than one language family. At the present time, the same phenomenon may be observed in the case of Ossetic, which shares many areal features with surrounding languages, though it is an Iranian language and not genetically related to its immediate neighbors (cf. Comrie 1981:158-179

for details).

Now, in view of the additional cognates listed in this paper, it may be possible that Proto-Northwest Caucasian belongs with Nostratic, if not as a daughter branch, possibly as a sister — it all depends upon the level at which one wishes to reconstruct a proto-language, upon how one sets up subgroups, and upon how one establishes the special relationships among members of the various subgroups. If this should turn out to be the case, then Proto-Indo-European and Proto-Northwest Caucasian would indeed be genetically related as descendants from a common ancestor, without necessarily implying a closer relationship. On the other hand, the lexical parallels in which cognates can be found in other Nostratic languages may simply be due to borrowing by Northwest Caucasian, which would mean that the case for genetic relationship would be substantially weakened.

NOTES

*I would like to thank Merritt Ruhlen for commenting on earlier drafts of this paper. His advice was extremely valuable and saved me from making a number of foolish mistakes. I alone am responsible for any errors that may exist in this paper. I would also like to thank Johanna Nichols for sending me a copy of the manuscript of her unpublished paper entitled "The Origin and Dispersal of Indo-European".

1) Here, I am using the term "blend" to conform with Colarusso — nowadays, the term "convergence" would be used to describe this kind of language contact.

2) The term "tectal" is being used as a replacement for the term "guttural", in accordance with current usage in Indo-European circles (cf., for instance, Lehmann 1993:19-20, 57, and 100-101).

3) Merritt Ruhlen has informed me (personal communication) that Starostin follows Illič-Svityč in positing a three-way contrast in obstruents for Proto-Altaic. As we were going to press, Ruhlen sent me a photocopy of Starostin's book — I deeply appreciate his kindness.

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IS PROTO-INDO-EUROPEAN RELATED TO PROTO-NORTHWEST CAUCASIAN?

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In a recent article, John Colarusso proposes that Proto-Indo-European (henceforth PIE) and Proto-Northwest Caucasian (henceforth PNWC) were descendants of a Proto-Pontic language that was spoken north of the Black Sea around 10,000 years ago (Colarusso 1992). His conclusion is that "PIE and PNWC are genetically related at a phyletic level." I personally have no doubts that PIE and PNWC are genetically related, and have shown as much in a joint article with John Bengtson (Bengtson and Ruhlen 1994). But I also have no doubt that Colarusso's mythical Proto-Pontic never existed — north of the Black Sea or anywhere else.

What Colarusso fails to realize is that these two families are related to one another without in any way constituting a valid linguistic taxon, or family. They are related to each other because they are members of larger families, which are themselves in turn related to one another. They are *not* related to each other because each of them is genetically closer to the other than either is to any other family of languages — and indeed such is not the case. It is worth noting that Sergei Starostin (1989) has also shown that PIE is related to PNWC, but his line of evidence is very similar to that of Bengtson and Ruhlen (1994) in that what he shows is that Nostratic is related to Sino-Caucasian — the first including PIE, the second encompassing PNWC — not that there is any *direct* connection between PIE and PNWC.

Colarusso's direct approach to distant genetic relationships is really a denial of the evolutionary process itself, for relationships, if they are anything but transparent, are not of the immediate nature that Colarusso envisages. No biologist doubts that pigs and humans are related, but it is not because pigs and humans derive from Proto-Porco-Human, but rather because pigs and humans belong to groups, which in turn belong to groups, which ultimately derive from a single group. No biologist would ever even think of the idea of comparing pigs and humans by themselves, much less actually do it. Further discussion of the nature of linguistic taxonomy — and its relationship to "traditional" historical linguistics — may be found in Ruhlen (1994a, 1994b).

This is one of the fundamental differences between biological taxonomists and historical linguists of the Eric Hamp school. It is certainly not surprising that Hamp appears to have inspired Colarusso's paper, for it is reminiscent of the putative Mayan-Chipaya, and Mayan-Mapudungu, hypotheses in the Americas, both of which were supported by Hamp (1967, 1971). According to the first, the Mayan family was alleged to form a family with the Chipaya language of Bolivia (Olson 1964, 1965), while the latter hypothesis (Stark 1970) connected Mayan with Mapudungu. When one approaches these problems more soberly (Greenberg 1987), it turns out that Mayan, Chipaya, and Mapudungu are each members of *different* branches of Amerind (Penutian, Equatorial, and Andean, respectively) and what they share is their common Amerind inheritance, not the special relationship envisaged by Hamp. These cases are directly parallel to Colarusso's Proto-Pontic and illustrate the absurdity of arbitrary binary comparisons.

What is most disturbing about Colarusso's article is that he operates as if nothing were known about the classification of the world's languages. His references cite none of the standard works on linguistic taxonomy (e.g. Greenberg 1957, 1963, Voegelin and Voegelin 1977, Ruhlen 1987), nor any of the abundant literature on the closest relatives of the Indo-Europeans, or of the Northwest Caucasians. In fact, he dismisses this literature in a footnote wherein he refers to "the folly of such grand lumping schemes" as Nostratic (or Eurasiatic) and Dene-Caucasian, without citing any of the relevant literature on these two families.

But one does not have to leap to the limits of Dene-Caucasian to see the folly of Colarusso's approach. It is now generally agreed that Northwest Caucasian languages are most closely related to Northeast Caucasian languages as the Caucasian family (Comrie 1981:198, Catford 1991:234). The earlier work of Trubetskoy (1930) and others had long ago established this fact, but the more refined and complete study of Nikolaev and Starostin (1992) demonstrates the relationship beyond any shadow of a doubt, and *this fact by itself* shows that Colarusso's hypothesis is incorrect in that PNWC is transparently closer to Proto-Northeast Caucasian (henceforth PNEC) than it is to PIE.

On the other side of the equation, the case is equally clear, though a good deal more controversial. Despite the fact that linguistic similarities among Eurasian families — many of which had already been noticed in the nineteenth century — led Holger Pedersen to posit, early in the present century, a Nostratic family that included Indo-European; and despite the thorough refinement and advancement of this hypothesis by Vladislav M. Illič-Svityč (1965, 1971-1984) and Aaron B. Dolgopolsky (1964, 1984), as well as the complementary work of Allan Bomhard (1991) on the same theme; and despite the similar results obtained by Joseph Greenberg using multilateral comparison to identify a Eurasiatic language family that is very similar to Nostratic (Greenberg, to appear), Indo-Europeanists, and Colarusso, act as if none of this evidence even exists. One might call it the Clinton model of historical linguistics, "Don't ask, don't tell, and lie about it if they find out." But this ostrich approach to historical linguistics cannot do away with — much less explain — the fact that the Indo-European pronominal pattern *m-/t-* 'I, you' is shared, in whole or in part, by numerous other Eurasian language families (e.g., Kartvelian, Uralic, Altaic, Korean, Japanese, Gilyak, Chukchi-Kamchatkan, and Eskimo-Aleut). Nor can it hide the fact that dozens of other grammatical formatives, and hundreds of other lexical items, are shared by these *same* families, and are absent from the world's other language families. Nor can it hide the fact that PNWC **sA* 'I' resembles such Northeast Caucasian forms as Proto-Nakh **sō*, Proto-Lezghian **zo-n*, and Xinalug *zi* far more than it does Eurasiatic **mi*. Nor does it conceal the fact that PNWC **wA* 'you' resembles Lak *wi-* 'you,' Proto-

Lezghian **uo-n* 'you,' Xinalug *wɨ* 'you', and probably Proto-Nakh **waj* 'we (inc.)' (if this form derives from a combination of 'you' + 'me,' a typologically plausible origin of inclusive pronouns) far more than it does Eurasiatic **ti*. Nor can it obscure the fact that both of these Proto-Caucasian pronouns are attested in other branches of Dene-Caucasian. The first-person pronoun is also found in Burushaski *ǰa*, Sino-Tibetan (e.g. Newari *ǰi*), Proto-Yeniseian **ʔadᶻ*, and Na-Dene (e.g. Proto-Athabaskan **šī*.) The second-person pronoun is seen in Burushaski *ū-n*, Proto-Yeniseian **ʔu*, and Na-Dene (Tlingit *wae*) .

Indeed, any objective taxonomist who compares Colarusso's feeble comparisons between PIE and PNWC with the robust Caucasian comparative dictionary of Nikolaev and Starostin (1992), and at further remove, the Dene-Caucasian comparisons of Starostin (1982, 1984), Nikolaev (1986), Bengtson (199la, b), Shafer (1952, 1957), and Čirikba (1985) on one hand, and the Nostratic/Eurasiatic comparisons, on the other, will find that Colarusso's comparisons are few and unconvincing when compared with the aforementioned body of evidence. Rather than using basic vocabulary, such as personal pronouns, body parts, and natural phenomena, Colarusso uses meanings such as "giant, cattle," and "(hard) metal" in his 20 "conventional cognates." Furthermore, the phonological connections are often less than transparent. Thus we are asked to believe that PIE **dwo-* 'two' derives from earlier stages: **t'əʔʷ* < **t'ʔʷə* < **t'q'o*, this latter form conveniently being identical with Proto-Pontic (and PNWC) **t'q'o* 'two.' If we are to believe all this, then PIE has undergone a whole series of radical sound changes during the same time that PNWC remained unchanged.

In sum, both the semantics and phonetics of Colarusso's comparisons are largely unconvincing. Some of the correct comparisons are in fact much more widespread than simply these two families and thus provide no evidence for Proto-Pontic. One such example is Colarusso's Proto-Pontic **pa* 'son, child.' This same root was posited for Nostratic by Illič-Svityč (1965) and for Sino-Caucasian by Starostin (1984), who also noted the similarities to the Nostratic forms in Starostin (1989). Greenberg (1987:199-200) showed that the same root is widespread in the Amerind family, and I have compared the Amerind material with the Eurasiatic forms (Ruhlen 1994c). In Amerind this root has undergone two interesting developments. First, the meaning has shifted from 'son, child' to 'brother'; and second, reduplication is used with the root to distinguish 'younger brother' from 'older brother,' as in Proto-Oto-Manguean **po* 'younger brother' vs. **papi* 'older brother.' In addition to these objections, Colarusso's Pre-PIE reconstructions are tortured and convoluted — as if their sole purpose was to make them seem similar to PNWC, perhaps not surprising in a paper that the author admits was initially inspired by typological similarities. It is a pity that in the late twentieth century such absurd proposals as Proto-Pontic are not only advanced by supposed experts, but are even discussed seriously by other supposed experts.

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COMMENT ON COLARUSSO 1994

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Editor Allan Bomhard has invited me, as a proponent of the Dene-Caucasian hypothesis, to comment on John Colarusso's hypothesis of a genetic link between Proto-Indo-European and Proto-Northwest Caucasian (hereafter simply West Caucasian or WCauc), which was reprinted in the last issue of *Mother Tongue*.

The hypothesis of genetic links between I(ndo-)E(uropean) and Caucasic languages dates long before 1964. C. C. Uhlenbeck (1937) thought that IE consisted of two distinct elements, A (which he identified with "Ural-Altaic", i.e., Nostratic) and B (identified with Caucasic languages in general, in the same sense Colarusso uses). To Uhlenbeck, Indo-European was presumably a creole language, or series of creole languages, that developed from the interaction of the A invaders with the indigenous B populations all over Europe. By the way, Uhlenbeck also connected the Eskimo language(s) with element A (Nostratic), and Basque with element B (Caucasic).

Anybody who is tempted by Colarusso's hypothesis should look at an article published by Prince Nikolai Trubetzkoy over six decades ago (Trubetzkoy 1930). This is the work that firmly established, in my opinion, the genetic link between West Caucasian and East Caucasian, on the basis of one hundred lexical comparisons. As in any pioneering study, there were some errors and incorrect comparisons, but we now have the good fortune of a recent edition (Trubetzkoy 1987) with corrections and additions by Sergei Starostin ((1987), making use of the new reconstructions of both families. Because these works are inaccessible to many readers of *Mother Tongue*, I will list some of Trubetzkoy's comparisons here. (Five comparisons, §§2, 6, 14, 17, 21, have been added. Basque, Burushaski, and other Dene-Caucasian forms are also cited, where relevant):

1. "I" (1st person singular): WCauc **sV* (Circ[assian] *sa*) = ECauc **zo* (Chechen *so*, Khinalug *zä*, etc.); cf. Burushaski *ǯa, ǯe*, (W[erchikwar]) *ža*; Yen(iseian) **ʔaʒ*; N(a)-D(ene): Athabaskan **ši*.
2. "two": WCauc **(t)q̓ʷV* (Circ *t'ʷə*) = ECauc **q'wą̈* (Lak *k'i*, Archi *q'ʷe*); cf. Basque *bi* (from **ɢʷi*, assimilated to *bat* "one", like English *four, five*, Latin *quattuor, quinque*); S(ino-)T(ibetan) **k-niy(s)*; Yen **xɨ-na*; ND: Athabaskan **qi*.
3. "thou" (2nd person singular): WCauc **wV* (Circ *wa*) = ECauc **u̯o-* (Archi *u-n*, Khinalug *vä*); cf. Burushaski *u-n*; Yen **ʔaw/*ʔu*; ND: Tlingit *wa-e*.
4. "you" (2nd person plural): WCauc **sʷə* (Circ *sʷa*) = ECauc **źwV* (Lak *zu*) = Basque *zu*; cf. Sumerian *za-, ze-, zé-* "thou", *-zu* "thy".
5. "what?": WCauc **sV* (Ubykh *sa*) = ECauc **sV* (Dargi *se*); cf. Basque *ze-r*; Yen **ʔas-/*sV-* (interrogative); ND:

Tlingit *daa-sa* "what", etc.

6. (interrogative): WCauc **-an(ə)-* "when?" = ECauc **nV* (Lezgi *ni* "who?"); cf. Basque *no-r* "who?", *no-iz* "when?"; Burushaski (W) *ana* "where, whither?"; Sumerian *a-na*, *en*, *en-na* "what"; ST: Tibetan *na* "when?"; Yen **ʔan-* "who".
7. "tongue": WCauc **bəźV* (Circ *bzə*) = ECauc **mɬc'i* (Andi *mič'i*, Tabasaran *melʒ*); cf. Basque *mihi*; Burushaski *melč* "jaw"; (?) ST **mlay* "tongue".
8. "name": WCauc **(p-)c'a* (Ubykh *p'c'a*) = ECauc **ʒwɬrʔɨ* (Andi *č'er*); cf. Burushaski *-čar* "voice".
9. "eye"; WCauc **b-ɬ'a* (Abaza *la*) = ECauc **ʔwilʕi* (Dargi *ħuli*, Chechen *bʕärg*); cf. Basque *begi* (from **berɣi*); Burushaski *-lči*, *-i·l-*.
10. "heart": WCauc **ǵʷə* (Circ *gʷə*) = ECauc **jerk'wi* (Lezgi *rik'*, Chechen *dog*); (?) Basque *bi-rika* "lung".
11. "tooth": WCauc **čə* (Circ *čə*) = ECauc **cɨɬħV* (Andi *sol*, Chechen *cerig*); Basque *hortz* [*horc̄*] (regular correspondence to metathesized Cauc **ħɨɬcV*);
12. "not": WCauc **mə-* (= Circ *mə-*) = ECauc **mV* (Chechen *ma*); cf. Burushaski *be*.
13. "louse": WCauc **ć'a* = ECauc **nɛmʒV* (Lak *nac'*, Chechen *mezi*); cf. Basque *bartz* "nit" (from **welʒe* > **wenʒe* > **menʒe* > **nemʒe*, by a series of assimilations and dissimilations; further Khinalug *nimc'* ~ *limc'* "louse"); the original shape **welʒe* or **welc'e* is confirmed by ND: Tlingit *wés'* "louse".
14. "die": WCauc **x̌'V* (Circ *x̌'ə-n* "to die, to kill") = ECauc **ʔi(w)x̌'V* (Hunzib *ix̌'a*); cf. Basque *hil*; ST: Old Chinese **x̌iy* "corpse"; ND: Haida *x̌'a-daa* "to kill" (with plural object).
15. "moon": WCauc **maza* (Circ *ma·za*) = ECauc **wɛmc'o* (Avar *moc̄'*, Lezgi *varz*); (?) Old Chinese **ŋʷat*.
16. "hand": WCauc **q́'a* ~ **q̊a* = ECauc **q'wɬV*; cf. Basque *u-khare*, etc. (cf. §22, below).
17. "night": WCauc: Abkhaz *-Xa* = ECauc (Lezgian) **X̄am*; cf. Basque *gau*, *gab-*.
18. "horn": WCauc **q̊́ʷa* (Ubykh *q́a*) = ECauc **qwä(hV)* (Lak *qi*, Dargi *qe*); cf. Yen **Xɔʔ*.
19. "full": WCauc **zV* (Ubykh *-za*) = ECauc **ʔoc'V* (Lak *b-uwc'u* "full", Chechen *d-uza* "to fill"); cf. Basque *oso* "whole, entire, healthy"; Yen **ʔute* "full"; ND: (?) Haida *st'ah* "full", Sarsi *-c'ist*.
20. "sun": WCauc **rəẏa* (Ubykh *dẏa*) = ECauc **(wi-)rɑɢV* (Lezgi *raɣ*, Archi *barq*); cf. Basque *argi* "light", *argi-zagi*, *hil-argi* "moon".
21. "ear": WCauc **l'V* (Abkhaz *-ləmħa*) = ECauc **lerħV* (Dargi *liħi*, Chechen *lerg*); cf. Basque *be-larri*.

Note that these comparisons all fall within Dolgopolsky's hierarchy of the twenty-five historically most stable meanings (Dolgopolsky 1964, 1986; Shevoroshkin and Markey 1986:xvii-xviii), the semantic fields that are the most likely to retain the genetic core of a language. The twenty-one comparisons listed here demonstrate the unity of WCauc with ECauc, Basque, and other languages in a macrofamily or macrophylum called Dene-Caucasian (Starostin 1982, 1984, 1989; Nikolaev 1991; Bengtson 1990, 1991a, etc.). One searches in vain for anything resembling the familiar Indo-European "tongue" (**dn̥ĝhū*), "eye" (**oqᵘ-*), or "tooth" (**edont-*)! (The Indo-European reconstructions are from A. Walde, *Vergleichendes Wörterbuch der indogermanischen Sprachen*.)

These comparisons stand in strong contrast to those of Colarusso, which include such meanings as "house" (§68), "giant" (§70), "cattle" (§73), "metal" (§§77, 78), all highly susceptible to cultural and areal diffusion. Indeed, of Colarusso's twenty "conventional cognates" (§§64-83), only one (§75 "two") falls within Dolgopolsky's hierarchy. His "morphological cognates" do include some personal pronouns (§33), but I find the comparisons tortured and implausible, compared with the straightforward parallels involving WCauc, ECauc, and other DC languages (see my §§1, 3, 4 above).

What about Colarusso's list of "morphological cognates", which Bomhard, in his comments on Colarusso's paper, finds convincing? Some of these "cognates" are of a general and typological nature (e.g., the opposition of thematic and athematic nouns and verbs) that could be found in any number of languages without being evidence of genetic relationship. Others are phonologically implausible, for example (as Bomhard admits), the comparison of IE **s-* with WCauc **(-y)-x̌ʰ-* (§52). Yet others may indeed be historically valid, but not, I think, indicative of a close relationship between IE and WCauc Some of these are reminiscent of comparisons made by Robert Shafer (1963, 1965) between IE and Sino-Tibetan. For example, the accusative *-m* (Colarusso §31) is found in the ST languages Mishing and Nyising (Shafer 1965:447, §2); and the IE sigmatic aorist (Colarusso §44) also has ST parallels (Shafer 1965:451, §16). Should "Pontic" or Nostratic then be extended to include ST as well?

I think a better solution is that of Starostin (1989), proposing a deep relationship between Nostratic (including IE) and Dene-Caucasian (including Cauc and ST), a proposal that is in fact a refinement of Shafer's "Eurasial". Starostin's comparisons encompass several of the parallels adduced by Colarusso (and Bomhard), for example, the negative/prohibitive particle **mV-* (Starostin 1989:64, §2) and the numeral "two" (Starostin 1989:62, §180). While I reject Colarusso's claim of a close genetic tie between IE and WCauc, I do not consider his efforts a total loss by any means. I think they can be useful for (a) typological and areal connections between IE and WCauc, and (b) evidence of a deeper genetic tie between Nostratic and Dene-Caucasian.

This problem is instructive, because it brings us back to the basic principles of genetic classification. Classification is not just a matter of piling up masses of etymologies and continually adding more languages to one's chosen family or macrofamily. The logical extension of Colarusso's results would produce a virtually endless string of languages, but no

clue as to the correct subgrouping. (The negative *mV* is found in Amerind as well. Do we then add Amerind to Nostratic?) The problem is to evaluate the parallels according to a hierarchy of stability such as Dolgopolsky's, or the Swadesh lists: "tongue", "name", "eye" are more indicative of genetic relationship than "house", "cattle", "metal"! When *only* words within the hierarchy are compared, it becomes quite clear that West Caucasian belongs to a group with East Caucasian (and, in my view, Basque and the other DC languages), while Indo-European does not.

Another principle involves structural compatibility. Edward Sapir (e.g., 1913, 1915) always insisted on establishing a structural pattern as the first step in the demonstration of genetic affinity. For example, in Dene-Caucasian there is abundant evidence of class markers which were prefixed to nouns, and also appear in some verb forms (Toporov 1971; Bengtson 1991a, 1991d, 1991e, 1993). This system is very much alive in many Cauc languages, in Burushaski, and in Yeniseian. In Basque (noun prefixes *a-*, *e-/i-*, *be-/bi-*, *o-/u-*) and in Sino-Tibetan (noun prefixes *a-*, *b-*, *r-*, etc.), these elements appear as "fossilized" and lexicalized remnants. This type of structure is alien to the early stages of Indo-European and other Nostratic languages.

Colarusso has not demonstrated that IE belongs in a family with WCauc (except, perhaps, at a remote level), because he has not shown that IE is closer to WCauc *than it is to other families*. We must not be fooled by typological similarities (remember the problems we had in Africa and Southeast Asia), nor by similarities that are too universal to be diagnostic (e.g., Bomhard's **k'ak'a* "to chirp", **hana* "mother", etc.). There have to be some agreements in basic vocabulary and grammatical structure.

As a coda, here are some Dene-Caucasian comparisons involving West Caucasian material. Several of them are drawn from "Lexica Dene-Caucasica" by Václav Blažek and myself (Blažek and Bengtson 1995), which we think further solidifies the comparative lexicon of the Dene-Caucasian macrophylum. The reader can judge whether these are more or less convincing than the WCauc-IE comparisons offered by Colarusso (and Bomhard):

22. WCauc **q̓'a ~ *q̓a* "hand" (Circ *ʔa*, Ubykh *q'ā-p'a*) / ECauc **q'weɬV* "hand" (Avar *q̄'wal*, etc.) / Basque *u-kharai*, *u-khare*, *u-kari* (combinatory form *u-k[h]al-*) "wrist" / Yen(iseian) **xɨre* "hand": Ket *ɨl'(i)*, Arin *kara-*, *koro-* (in compounds) / (?) S(ino-)Tibetan: Tibetan *khyor* "palm, handful" (Starostin 1982:209).
23. WCauc: Circ **ş^{w}t̄ə* (Adyghe *ş^{w}t̄ə*, Kabardian *ftə* "genitals") / Basque *iztai* "anus", *izter* "thigh" / Burushaski *-aṣ̌ç̌-iŋ*, (W) *-aṣ̌ṭ-iŋ* "small of the back, loins, reins, waist" / ST: Tibetan *sta-zur* "hip, hipbone" (entire comparison by Bouda 1950, §204 and 1954, §19).
24. WCauc: Abkhaz *-k'ək'a-* "female breast" / ECauc **kɨ̲k[u]*: Hunzib *kigla* id., Lak *k̄uk̄u* "nipple" / Basque *kholko*, *golko* (from **khoklo* ?) "breast, bosom" / ND: Haida *k'u·k* "heart", Tlingit *kegú* "lungs" (Čirikba 1985:99, §44; Blažek and Bengtson 1995, §23).
25. WCauc **č̓'ač̓'a* "kidney" (Abkhaz *a-č̓'ač̓'a*, Kabardian *źa źaj*) / ECauc **ć'e ć'V* (Dargi *ur-cec*, Khinalug *c'ic'in* "kidney") / Yen **sisa(l)-* "lights" (lungs) (Kott *šiča·tn*, Arin *šišali*) / ND: Haida *ča·y* "kidney"; Athabascan: Sarsi *c'ùzą́* id. (Starostin 1982:224; Blažek and Bengtson 1995, §28).
26. WCauc: Circ **č̓'əş^{w}a* (Adyghe *č̓'əş^{w}a*, Kabardian *ş'əfa* "human skin, body") / Yen **sä·s* "(animal) skin": Ket *śa·śi*, Kott *še·t* / ND: Tlingit *ʒàs* "skin"; Eyak *-sic'* "fish skin"; Athabaskan: Galice *-sa·s* "(animal) skin", Hupa *sɪc'* "skin, bark" (Blažek and Bengtson 1995, §36).
27. WCauc **ła* "foot" (Ubykh *ła*) / ECauc **łeɬ̄V* (Avar *łar-k'* "sole", Chechen *lar* "trace") / Basque **loī-* in *lorr-ats* (Vizcayan, Guipuzcoan) "track, trail, trace, scent" / ST **lăH* "foot" (Starostin): Chepang *la*, Dimasa *ya*) / ND: Eyak *łą·ʔ* "thigh" (Starostin 1984; 1991, §19; Blažek and Bengtson 1995, §41).
28. WCauc: Circ **k^{h}əx^{w}* (Kabardian *kəx^{w}* "dry twigs") / N(a-)D(ene): Haida *kuk* "firewood"; Eyak *kuk-ł* "dry wood, firewood", Navajo *čīž*, etc. (Blažek and Bengtson 1995, §71).
29. WCauc **k'ank'a* "egg" / ECauc **k'erk'enV* (Andi *k'ork'on* "egg", Avar *k'ork'onu* "grape, berry") / Basque (Vizcayan) *kankano* "large fruitstone, kernel, almond" / Burushaski *kaka·yo* "(walnut) kernel" / ND: Haida *k'á·nk'a·y ~ k'á·nk'a·n* "unripe berries"; Athabaskan: Navajo *-k'ǫ́·ʔ* "seed, pit" (Blažek and Bengtson 1995, §74).
30. WCauc: Circ **q'aş'^{w}ə* "sweet" / ECauc **q'(w)Vlć'ć'V* "sour" (Lak *q'urč'-*) / Basque *gozo ~ goxo* [*gošo*] "sweet, tasty" / Burushaski *gaš-ar-um* "salt-sweet" / Sumerian *kuš$_{6}$* "honey, sweet" /ND: Eyak *q'ihǯ* "rancid, bitter, sour, spoiled"; Athabaskan: Navajo *k'ǫ̨š*, *-k'ǫ́·ž* "sour, salty, brackish", Mattole *-k'o·ʔǯ* "sweet" (Nikolaev 1991:57, §11.12; Blažek and Bengtson 1995, §187).
31. WCauc: Abkhaz *-k^{w}a* (plural of "non-reasonable" class) / Basque *-k* (noun plural suffix) / Burushaski *-ko(ŋ)* (plural of certain nouns) / ND: Tlingit *-x'*, *-x'^{w}* (plural or collective suffix); Athabaskan: Navajo *-ke*, *-kei* (noun plural suffix) (Dumézil 1933:135; Blažek and Bengtson 1995, §212).
32. WCauc: Abkhaz *-as*, *-s* (instrumental suffix) / ECauc: Chechen *-s* (animate ergative), Hurrian *-(u)s* (ergative) / Basque *-z*, *-ez* (instrumental) / ST: Tibetan *-s* (instrumental), Dhimal *-so* id., Kanauri *-s* (agent) / Yen: Ket *-aś* (instrumental-comitative), Kott *-os* (comitative), *-s(e)* (instrumental) (Tailleur 1958:418; Čirikba 1985:95; Blažek and Bengtson 1995, §213).

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INDO-EUROPEAN AND URALIC TREE NAMES

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1. INTRODUCTION

In this paper I present a number of similarities in tree names shared by Indo-European (IE) and Finno-Ugric (FU) or Uralic (U) languages. These shared terms are sufficient to demonstrate some historical connection between the two language families — due either to genetic relationship or language contact, or perhaps both.[1] The purpose of this paper is to present the evidence from tree names that the two language families have a very old historical connection, whether genetic or diffusional, and in doing so hopefully to stimulate further research towards determining the exact nature of the connection.

Both genetic relationship and areal (Sprachbund) affinities have been proposed which include these two families. Several proposals of distant linguistic relationship which link U to IE have been made. The Indo-Uralic hypothesis continues to be controversial, though attractive (see Collinder 1934, 1954, 1965; Joki 1973; Uesson 1970). Broader proposals of remote genetic relationship are looked on favorably by some, but skeptically by others. These include Nostratic (which,

whether in wider versions or narrower views, all include both IE and U) (see Dolgopolsky 1984; Kaiser and Shevoroshkin 1988; Shevoroshkin 1989; etc.) and Eurasiatic (see Greenberg 1987 and forthcoming; Ruhlen 1987 and 1990), etc. Should any of these proposals of remote genetic relationship prove well founded, some of the shared tree names presented here may turn out to be cognates.[2]

On the other hand, research on possible distant genetic relationships involving IE and U have tended not to take into consideration the implications of areal linguistics. Various proposals for linguistic areas (Sprachbünde) involving IE and U, along with various other languages and families, have been made (see, for example, Décsy 1988; Haarmann 1976; Friedrich 1970; Jakobson 1938; Joki 1964 and 1973; Katz 1975; Sebeok 1950; Sinor 1969; Thomason and Kaufman 1988; Zeps 1962). Given these proposed linguistic areas and documented early loans from IE languages found in FU languages (cf., e.g., Joki 1973; Koivulehto 1976, 1980, and 1984; Korenchy 1988; Rédei 1986-1988; Suhonen 1988; Tauli 1955), it is possible that some of the similarities presented here may well turn out to be old loan words due to intimate contact between PIE and PFU or PU languages.

It is hoped that the evidence assembled here from tree names for an old connection between IE and FU (or U) may help in further explorations of the competing (and perhaps overlapping) explanations offered by distant genetic and areal linguistic proposals for the similarities shared by IE and U languages. In particular, given the evidence for diffusion presented here, approaches to proposed distant genetic relationships involving IE and U should be modified to include the implications of areal linguistics. That is, while areal linguistic considerations are given importance in remote genetic research in other areas of the world, e.g., among American Indian languages, in south and southeast Asia, and in Australia, areal concerns have not played as important a role as they should in proposals which attempt to connect IE (and FU to a lesser extent) with other languages.

2. BACKGROUND

The subject of tree terms is of considerable importance in the literature on both IE and U, since arguments for the original homeland of each of the two families rely heavily on the tree names (see below).[3]

The classification of U languages is:

Uralic Family

I. Finno-Ugric Division	*Population*
1. Early Balto-Finnic Group	
a. Late Balto-Finnic	
Finnish	5,000,000
Estonian	1,000,000
Karelian	138,000
Veps	16,000
Votic	a few dozen
Livonian	300
b. Lapp	38,000
2. Votic Group	
Mordvin	1,200,000
Cheremis (or Mari)	622,000
3. Permic Group	
Zyrian (or Komi)	478,000
Votyak (or Udmurt)	714,000
4. Ob-Ugric Group	
Ostyak (or Khanty)	21,000
Vogul (or Mansi)	7,600
5. Hungarian	14,000,000

II. Samoyed Division	*Population*
1. Northern Branch	
Nenets (or Yurak)	25,000
Enets (or Yenisei Samoyed)	400
Nganasan (or Tavgi)	1,000
2. Southern Branch	
Selkup (or Ostyak Samoyed)	4,300

(After Korhonen 1984 and Hajdú 1975a, b)[4]

3. TREE NAMES AND HOMELAND RESEARCH

The linguistic study of the Finno-Ugric (and Uralic) homeland has an ample history, and there have been several different opinions concerning its location and even its nature (see E. Itkonen 1966:22-31). Tree names have played a very significant role in this research. All the linguistic evidence for the Finno-Ugric homeland points to an area located in the Middle Volga or in the area between the Volga and the Urals (Korhonen 1984:61; T. Itkonen 1984:350). Proto-Finno-Ugric vocabulary offers limited clues for delimiting the homeland, and tree names have been crucial.

The five principal trees involved are:

1. "spruce" [Picea obovata], PU ***kåxsɨ*, PFU **koosi* (Sammallahti 1988:538), with cognates in Balto-Finnic, Lapp, Mordvin, Cheremis, Votyak, Zyrian, Ostyak, Vogul, Nenets, Enets, Nganasan, Selkup, and Kamas.
2. "Siberian pine" [Pinus sibirica], PU ***sɨksɨ*, PFU **sɨksi* "cedar" (Sammallahti 1988:540); PU ***sikse* "Siberian pine (Pinus sibirica)" (Hajdú 1979:37); with cognates in Votyak, Ostyak, Vogul, Selkup, and Kamas (see below).
3. "Siberian fir" [Abies sibirica], PFP **ńulka* "silver fir" (Sammallahti 1988:553); PU ***ńulka* "Abies sibirica" (Hajdú 1975a:37); with cognates in Votyak, Zyrian, Ostyak, Vogul, Selkup, and Kamas.
4. "Siberian larch" [Larix sibirica], PFU **ńäŋe* "Siberian larch" (Hajdú 1975a:37); with cognates in Zyrian, Ostyak, and Vogul.
5. "brittle willow" [Salix fragilis]/ "elm", PFU **śɨliw* "elm"

(Sammallahti 1988:549); PFU *śala "elm, brittle willow (Salix fragilis)" (Hajdú 1979:38; Collinder 1960:413), with cognates in Finnish, Mordvin, Cheremis, and Hungarian; outside of Finnish, the cognates mean "elm" [Ulmus]. (Cf. Hajdú 1969:257 and 1975a:37-40).

The ancient distribution of these trees has been studied from their pollen in layers of peat and silt from the bottom of swamps. All except the "elm" have been part of the Siberian flora from the earliest times. Nevertheless, the "spruce" spread already during the Early Holocene over the Urals as far as the White Sea. The "Siberian pine" and "Siberian fir" appeared in Europe in the Middle Holocene (6,000 - 500 B.C.), but only in the eastern part bordering the Urals, the upper reaches of the Petšora; the "Siberian fir" also reached the Kama River. The "Siberian larch" spread from the Urals to the west only around 500 B.C., but it was found in western Siberia already in the Middle Holocene. The "elm", on the other hand, with its western origin, began to spread in the Early Holocene from the deciduous forests of Central Europe to the east and met the Siberian conifer forest on the west side of the Urals in the Middle Holocene. According to Hajdú (1964, 1969, 1975b), the Finno-Ugric homeland could be located only in an area where all these trees were found. The only place which fits temporally and geographically is from the Middle Urals to the north, including the lower and middle course of the Ob and the headwaters of the Petšora in the area of the northern Urals.

Evidence for the original homeland has also been sought in contacts with other languages. Proto-Finno-Ugric has a significant layer of loans from Indo-European, specifically Indo-Iranian. The contacts that led to these loans could scarcely have happened anywhere else than in eastern Europe (cf. Joki 1973). One such example is "honeybee". From the beginning (Köppen 1886, 1890), the words for "honeybee" (*mekše: Finnish *mehi-läinen*, Mordvin *m'ekš*, Votyak *muš*, Zyrian *moš*, Hungarian *méh*) and "honey" (*mete: Finnish *mesi*, Lapp *mieta*, Mordvin *m'ed'*, Cheremis *mü*, Votyak *mu*, Zyrian *ma*, Hungarian *méz*) have been emphasized in studies of the FU homeland. These terms were borrowed into PFU from IE, apparently from Proto-Indo-Iranian. The area in which such contact could have taken place was the region of the middle course of the Volga River, where apiculture was practiced in one form or another from the earliest times (Korhonen 1984:61; cf. Hajdú 1975b:33).[5]

To sum up the Urheimat considerations, the linguistic evidence available strongly suggests that the Proto-Finno-Ugric (and Proto-Uralic, as well) original homeland was located in the area somewhere between the Volga and the Urals, and may have extended a bit over the east side of the Urals. Nevertheless, Finno-Ugric-speaking people may have come far to the west, as far as the eastern Baltic region, before the break-up of the unified proto-language (Korhonen 1984; cf. T. Itkonen 1984:350; Sammallahti 1984). It is hypothesized that Proto-Uralic was spoken in the Middle Holocene when the spruce, Siberian pine, and Siberian fir had already appeared in Europe, or at least in the area of the Urals, i.e., after 6,000 B.C. On the other hand, the Proto-Finno-Ugric period could not begin before the elm's spread had reached the Siberian conifer forest's western edges, ca. between 6,000 and 4,000 B.C. (Hajdú 1964 and 1969).[6]

It will not be overlooked that this postulated FU homeland is quite near that assigned to PIE speakers in most proposals. It is not surprising, then, that FU (also U) and IE speakers would have known the same trees, although it is more significant that their names for these trees are in several cases quite similar. With this background in mind, I now turn to a comparison of tree names in these two language families.

4. COMPARISON OF TREE NAMES[7]

4.1. *Cedar*

Friedrich (1970:149) lists as possibly connected and worthy of further study the IE forms:

Greek	κέδρος
Lithuanian	kadagỹs
Latvian	kadęgs (SKES 170)
Old Prussian	kadegis "juniper" (Rédei 1986-1988:165)

It seems, however, that these are borrowed from FU languages, at least the Lithuanian and Latvian forms (SKES 170). They can be compared to the FU forms:

PFU *kečŋe (Collinder 1960:129 and 140), PFU *käčз "juniper" (Rédei 1986-1988:133)

Finnish	kataja
Veps	kadag
Vote	kataga
Estonian	kadak(as), kadaj(as)
Livonian	kadàg
Zyrian	kač-
Proto-Lapp	*kęsŋęs (kaskas, gasskas, gasŋes) (Lehtiranta 1989:42)
Vogul	kaštaj-, kašē-
Ostyak	kočŋi, hašŋa
Selkup Samoyed	keča, kyča
Kamas Samoyed	keedəmgə
Koibal (Samoyed)	kaduma (Collinder 1960:129; SKES 170)

The borrowing probably is from the Balto-Finnic languages; it is uncertain whether these Balto-Finnic forms are actually cognate with the others listed for which Collinder has reconstructed the PFU form listed here. It is conceivable, nevertheless, that these later cognates and the Greek form may also have connection, although not as recent (and by no means as obvious) as the Baltic borrowing from Balto-Finnic.[8]

4.2. *Willow*

One IE term for "willow" is reconstructed as *sVlyk- (Friedrich 1970:53), *sal(i)k- (Watkins 1985:56), with

cognates:

Greek	ἑλικών, ἑλίκη (in Arcadian, 6th century B.C. Ϝελικων)
Latin	salix
Old Irish	sail
Mid. Welsh	helygen
Old English	welig
OHG	salah(a)
Old Norse	selja
Proto-Gmc.	*solk-
Anatolian (?)	ulelku- "grass"

The **salyk-* cognates are in Western Indo-European; to accommodate the forms with *w*, Friedrich posits a western and central **s/wVlyk-*. Also, the "willow" forms may be etymologically related to "grass" (Friedrich 1970:55 and 155).

There are two FU cognates sets which compare well to these IE forms; the first is:

PFU **śɨliw*, PFP **sëlv*, PUg **śɨliw* "elm" (Sammallahti 1988:549);
PFU **śala* "elm, brittle willow (Salix fragilis)" (Hajdú 1979:38; Collinder 1960:413);
PFU **śala* "elm" (Rédei 1986-1988:458):

Finnish	salava "willow" (dialectal *halava*)
Mordvin	śel'eŋ, śel'ej; śäl'i "willow"
Cheremis	šo(·)l "elm"
Hungarian	szil "elm", szilas "elm grove"

(SKES 954; Hajdú 1969:257 and 1975a:38; Collinder 1960:413)

This FU set is strikingly similar to the IE etymon, though the FU word probably originally meant "elm", which shifted to "willow" in Finnish (E. Itkonen 1969:303). The "elm", with its western origin, began to spread in the Early Holocene from the deciduous forests of Central Europe to the east and met the Siberian conifer forest on the west side of the Urals in the Middle Holocene (see above; Hajdú 1969 and 1975b).

A comparison of the IE "willow" set with the FU forms should not occasion too much skepticism, since, in spite of the "elm" meaning of the FU forms, "willow" is clearly involved in the cognate set. That is, given the semantic shift within FU involving the meanings of "elm" and "willow", a similar semantic shift in some of the forms compared between FU and IE is not implausible.

The other U etymon which can be compared with the IE form is PFU **śɨlkå*, PUg **sɨla* "pole" (Sammallahti 1988:549); PFU **śalkɜ* (Collinder 1960:413); PFU **śalkɜ* "pole, stick, staff" (Rédei 1986-1988:460):

Finnish	salko "thin, long pole, staff, stick, stake"
Mordvin	śalgo "stick, pin"
Vogul	sailä, saila "slat, lath, splint"
Ostyak	sāγət, sāγəl, sa·xə̑l "splinter, lath, picket"

(SKES 955-956; Collinder 1960: 413)

While semantically a comparison of "willow" with "thin pole" is not as compelling as semantically equivalent forms would be, these are, nevertheless, sufficiently similar in meaning to invite curiosity about possible connections.[9]

4.3. *Poplar, aspen*

PIE **osp-* (Friedrich 1970:49) is attested in three subgroups (cf. Watkins [1985:3] **apsā* "aspen"):

Old English	æspe "white poplar, aspen"
North Sorbian	wosa "white poplar"
Lithuanian	apušẽ, epušẽ "black poplar"
Latvian	apse "quaking asp"
Russian	osína "quaking asp"
Proto-Slavic	*opsa "aspen"

To these, the following additional but problematic IE forms can be compared (Friedrich 1960:50-51):

Armenian	op'i "white poplar" (fit not explicated)
Greek	ἀσπίς "shield" (improbable cognate)
Sanskrit	sphya- "front oar on a boat, boat pole, shoulder blade, sacred instrument"
Wakhi (Iranian)	fiák "shoulder"
Persian	fah "oar, paddle"
Proto-Indo-Iran.	*sphiya- "oar, shovel, shoulder blade"

There are several interesting aspects of this etymon, even with attention restricted to the first, less controversial set of forms. Indo-Europeanists disagree about the sequence of medial consonants, and some think the word looks non-Indo-European (Friedrich 1960:49). Friedrich points to similar forms for "Populus" in several languages of southern Siberia:

Tobol	ausak
Altai	apsak
Lebed	apsak
Kumandu	aspak
Chuvash	ëvës
Sagai	os, ōs

From this evidence, Friedrich (1970:50) concludes:

> during the third and second millen[n]ia both Indo-European and non-Indo-European languages shared forms with both orderings of medial consonants that referred to poplars and aspens. *Osp-* was an areal term shared by members of a *Sprachgruppe* of interacting speech communities in eastern Europe and southern Siberia.

Friedrich (personal communication, 1975) intends the *Sprachgruppe* to include FU and IE; this would be clear in any case based on the similarities exhibited in the FU names for this tree. We can compare: PFV **šapa* "aspen, poplar" (Rédei 1986-1988:783):

Finnish	haapa "aspen, poplar"
Proto-Lapp	*supē "aspen, poplar" (Lehtiranta 1989:126)
Cheremis	ša·pki, ša·pi, šopke· "aspen, poplar" (SKES 46)

4.4. *Another U "aspen/poplar" term*

Uralic has another, more extensively attested etymon for "poplar, aspen", which also bears considerable similarity to IE and other forms already cited above. The reasons for associating the IE and these U forms are quite obvious. Nevertheless, the U data are a bit cumbersome. It seems that originally there may have been perhaps two distinct but phonetically similar etyma, one for "aspen/poplar" and another for "willow", which have in some ways merged, either in the actual languages or at least in the minds of those presenting the forms. It becomes very difficult to separate them, if indeed they were originally distinct. I present both here:

PU ***poje* "aspen, poplar (Populus)" (Hajdú 1979:37); PU ***poja* "aspen (Populus tremula)" (Décsy 1990:106); PFP **poji* "aspen" (Sammallahti 1988:553); PU ***pojз* "aspen" (Collinder 1960:408). PU ***påjiw*, PFP **pajiv*, PUg **poji-* "willow" (Sammallahti 1988:548); PU ***paje* "willow (Salix)" (Hajdú 1979:37); PU ***paja* "a kind of Salix" (Décsy 1990:105); PU ***pajз* "willow species" (Rédei 1986-1988:349), PU ***pojз* "aspen" (Rédei 1986-1988:391):

Finnish	paju "willow"
Mordvin	poj, poju "aspen, poplar"
Votyak	bad′ "willow"
Zyrian	bad′, baid "willow"
Ostyak	poi̯, pai̯ "aspen, poplar"
Yurak Samoyed	p′èβ "basket with willow inside"
Ostyak Samoyed	p′è, py "willow basket"
Votyak	pi(-pu) "aspen"
Proto-Samoyed	*pi "aspen" (Janhunen 1977:123)
Yurak Samoyed	pi "aspen"
Selkup Samoyed	pi "aspen"
Kamas Samoyed	pii-ní "aspen"[10] (SKES 465-466)

4.5. *Conifer species*

Several terms for conifers will be considered. The first is a "pine" term. As a "weak and chronologically late PIE tree name denoting some sort of conifer, probably the pine", Friedrich (1970:34) reconstructs **pytw*, based on these forms:

Greek	πίτυς "pine, spruce"
Latin	pīnus "pine, fir" (< *pīt-snus or *pīt-snos)
Sanskrit	pítu-dāru- "a kind of pine, fir, or resinous tree" (Some have suggested a Dravidian origin for this, compare Telegu *pisunu*, originally "gum, resin" [Friedrich 1970:34].)
Albanian	pishë "pine, fir" (related to *peshk(ve)* "resin, tar") (*pishë* perhaps from **pīt-s-i̯a*)

This reconstruction is supported by three Mediterranean stocks and doubtful Indic. Benveniste (1955) did not think it was part of the common IE lexicon. Friedrich (1970:34) points to "alleged loans" into Balto-Finnic (e.g., Finnish *petäjä*) "as possible evidence of its antiquity". However, this form has a fairly respectable antiquity in FU, going back at least to Proto-Finno-Permic times. Some forms supporting this are:

PFP **pečä* "pine" (Sammallahti 1988:553); PFP **pečä*, **penčä* "Pinus silvestris" (Rédei 1986-1988:727)

Finnish	petäjä "pine"
Proto-Lapp	*pēcē "pine" (Lehtiranta 1989:100)
Mordvin	p′itšɛ "pine"
Cheremis	pəndžə, pündžö, penže "pine"
Votyak	pužim "pine"
Zyrian	požem "pine" (SKES 534)

Hajdú (1964:54 and 1979:37) finds a possible, though uncertain Hungarian cognate *fenyő* "pine"; he reconstructs PFU **peńe* "Pinus silvestris" ("pine"). Rédei (1986-1988:416) reconstructs PFU **pönз* "fir, spruce", based on such cognates as:

Cheremis	piń "zum Pflanzen gelassene Fichte"
Votyak	pumel′ "fir"
Zyrian	ponu·l′ "young fir, juniper"
Hungarian	fenyő "fir, spruce, pine"

The fact that the form is only weakly attested in IE lends support to those inclined to suspect a borrowing from FU. However, the lack of clear and uncontroversial cognates beyond Finno-Permic within FU would tend to sober too much enthusiasm for a facile assumption of FU-to-IE borrowing. On the other hand, forms similar to these from Turkic and other so-called "Altaic" languages suggest wider areal diffusion,

compare:

Yakut (Turkic)	bäs "Scotts pine"
Soyot (Tuva)	pöjš "Pinus cembra" (Räsänen 1955:26)

The connections with "resin, pitch" (see below) may help to sort out the history of these terms.

4.6. *Second "conifer" connection*

The evidence for historical contact between the two families is strengthened by the close agreement among other conifer terms. A second of Friedrich's IE conifer terms, bearing a certain phonetic similarity to the first, is supported by three stocks — **pyk-* (Friedrich 1970:35), with the following cognates:

Greek	πίσσα "resin, tar"
Latin	pix, picis "pitch, tar, pine"; picea "spruce"
Umbrian	peiu "spruces" (< *pik-i̯e-)
Old Ch. Slavic	рьсьлъ "pitch, tar"
Serbo-Croatian	pàkao "pitch, tar"
Polish	piekło "pitch, tar"

These (as well as the Albanian forms in 4.5 above) compare well with the FU forms:

PFU **piška* "resin, pitch, gum" (Collinder 1960:413); PFU **piška* "resin" (Rédei 1986-1988:385-386):

Finnish	pihka "resin, pitch, gum"
Veps	pihk "pitch, fir, dense forest"
Vote	pihku "pine, pitch"
Ostyak	p'ĭγəᾳ, pìγl̥i`ţä`' "to plug with pitch, to tar a boat"
Votyak	pi`γl̥iţä "to plug with pitch, to tar a boat" (SKES 541-542)

The similarity in these last two cases between the PIE and FU forms would seem to be striking enough to make chance an unlikely explanation.

4.7. *Third conifer term*

The reconstruction of a third IE conifer term is supported by four subgroups. It is **pwḱ-* or **pewḱ-* "pine, spruce", with cognates:

Greek	πεύκη "spruce, pine, spruce forest"
Lithuanian	pušìs "spruce, pine"
Middle Irish	ochtach "kingpost of a house" (< **puḱtātā*); octgag "pine" octhgacha "fir"
OHG	fiuhta, fiehta "spruce"

This set, with somewhat limited distribution within IE, compares well to U terms for "tree", where in the U area "spruce and "pine" are the most typical of trees:

PU ***puxɨ*/***poxɨ*/***päxɨ* (final ***i* or ***ɨ*), PFU **puxi*, PS **pä*, PU **pugi* "tree" (Sammallahti 1988:539); PU ***pu* (Collinder 1960:408); PU ***punga* "tree, wood" (Décsy 1990:106); PU ***puwe* "tree, wood" (Rédei 1986-1988:400):

Finnish	puu "tree, wood"
Cheremis	pu "tree"
Votyak	pu, puш "tree"
Zyrian	pu "tree"
Vogul	-på "tree"
Hungarian	fa "tree"
Proto-Samoyed	*pä "tree, forest, wood" (Janhunen 1977:117)
Yurak	p'å "tree"
Yenisei	pē "tree"
Tavgi	fā "tree"
Ostyak Samoyed	pō, pū "tree" (SKES 664)

There is also another U etymon which can be compared with the IE forms, though I find it only in Collinder:

PU ***pükä* "cone of conifer" (Collinder 1960:408)

Cheremis	pügəl'mö "cone of conifer"
Ostyak	pöki "seed of a coniferous tree"
Selkup Samoyed	puuga, puuka, püügä "seed of a fir tree" (Collinder 1955:53)

4.8. *Fourth "conifer" term*

Hajdú (1969:257) cites another possible conifer connection between U and IE. He gives IE **sukse-* "pine (Pinus cembra)", which he compares with the PU ***sukse* of the same meaning. I am uncertain concerning this IE form, but find the following IE and U cognate sets quite comparable:

PIE **s(u̯)ek^{u}o-s* "resin, sap", Watkins (1985:68) **s(w)okwo-* "resin, juice":

Greek	ὀπός "resin, sap"
Latvian	svakas "resin, sap"
Lithuanian	sakaĩ "resin, sap"
Russian	osoka "Bluteiter"
Albanian	gjak "blood" (Pokorny 1959:1044)

And:

PU ***sɨksī*, PFU **sɨksi*, PS **tɨtə*, PFP **sëksi*, PUg

*θɨkθi "cedar" (Sammallahti 1988:540); PU **sikse "Siberian pine (Pinus sibirica) (Hajdú 1979:37); PU **soksa/**seksä "Pinus cembra" (Décsy 1990:107); PU **soksɜ (**saksɜ, **sẹksɜ) "Pinus cembra" (Rédei 1986-1988:445):

Votyak	susi-(pu) "juniper"
Zyrian	sus-(pu) "cembra pine"
Vogul	tēt, tāt "pine" (*t-* < **s-*)
Ostyak	tēxət
Yurak Samoyed	tydõ ?
Selkup Samoyed	tyty
	(Hajdú 1969:257)[12]

4.9. *Fifth conifer*

Another conifer listed under Friedrich's (1970:151) "Miscellanica Arborea" as "pine, cedar" is illustrated with the IE forms:

Armenian	mair "pine, fir, cedar"
Latvian	mītra "boxtree"
Old Ch. Slavic	smrěčъ "juniper"
	smrěča "cedar"

These can be compared with such FU forms as:

PFU *morɜ "a species of tree" (Rédei 1986-1988:281)

Proto-Lapp	*mōre "tree" (Lehtiranta 1989:78)
Hungarian	mór "spruce"

As for other languages with possible areal connections, Tungus dialects have *mar* "spruce" (Sinor 1969:280).[13]

4.10. *Apple*

Terms for "apple" seem to represent a true *Wanderwort* in northern Eurasia. I will consider first the IE and FU terms, and then those from other languages that seem to be involved. Friedrich (1970:57) suggests two forms for IE "apple", **abVl-* and **maHlo-*, saying that these rest "to an unusual degree on external, nonlinguistic criteria". The first has the supporting forms:

Pre-Celtic	*abalos "apple"	*abal-n- "apple tree"
Old Irish	ubull "apple"	
Middle Welsh	afallen "apple tree"	
Old English	æppel "apple"	apuldor "apple tree"
Pre-Baltic	*ābūl "apple"	*ābeles "apple tree"
Latvian	âbuolis "apple"	âbels "apple tree"
Old Ch. Slavic	(j)ablъko "apple"	(j)ablanъ "apple tree"
Russian	jábloko "apple"	jáblonъ "apple tree"

On the basis of these forms, Friedrich (1970:58-59) posits a North Indo-European **abVl-*. This reconstruction is peculiar, however, in that it has the otherwise very rare IE *b* and a divergent *l*-stem.

Joki's (1963 and 1964) survey of "apple" terms in various language families of the region (together with different opinions held concerning these) is instructive here. It might be noted that Sanskrit *phála-m* "fruit" has been thought by some to be cognate with the apple terms, while others have thought the North Indo-European terms may be borrowed. Finally, the Iranian forms have been much discussed (cf. Friedrich 1970:63; Joki 1963), as seen in:

Wakhi	mür, mü·r "apple"
Zaza	meróe "apple"
Pahlavi	mur
Modern Persian	mul "poor wild pear"
Shughni	mun, mūn "apple"
Sar(i)koli	man "apple"
Münjani	amiŋgá "apple"
Proto-Iranian	*amana/*amara "apple"
	(Joki 1963:135)

Morgenstierne (as cited by Joki 1963:135) in his Afghan etymological dictionary gave: *mana*, *māna* "apple", *mānū* "chokeberry (?)", and *mara ūna* (pl.) "sour apple", and derived all from **marna-*. In addition to those already cited above, he compares:

Sarkoli	man "apple"
Ishkashmi	mind "apple"
Jidghan	amunoh "apple"
Parachi	amar "apple"
Ormuri	milīz, miīć "apple"

The following FU terms can be compared with these from IE, as well as with the forms in other languages to be presented below:

PFV *omena ~ *omɜrɜ "apple" (Rédei 1986-1988:718)

Finnish	omena "apple" ("pear" also in dialects)
Estonian	omin, ubin, õun(a) "apple"
Livonian	umàr "apple"
Mordvin	umal, mal "apple, fruit, berry"
	mafl'u "apple tree"
Votyak	umo "apple" (probably from Chuvash) (Rédei 1986-1988:718) (SKES 429-430)

Some have suggested that FU may have borrowed from Old Iranian or the "Scythians" (Joki 1973), while the borrowing may also have been in the other direction. However,

the picture becomes more interesting when the forms for the following additional languages from northern Eurasia are taken into account:

Chuvash	olma, omla, uma "apple"
Mongolian	alima, aliman, alim "apple"

Räsänen (as reported by Joki 1963:135) thought these were from **alyma* with a prothetic **a-* and that they were perhaps borrowed from Sino-Korean **lim*. It is usually assumed that Votyak **umo* "apple" is borrowed from Chuvash (SKES 430).

Basque terms have also been compared: *mandaka* "sour apple species" (< **mantaŋka*), *mandaitu* "a kind of apple", *amun* "a kind of apple" (< **a-mont*) (Joki 1963:135).

Joki investigated all these "apple" terms in detail, concluding that the Turkic and Mongolian "apple" terms are also cultural loans spread widely from one language to another, whose probable origin was the fruit areas of or near Tajikistan (Joki 1963). Since there has been such widespread borrowing, and since the reconstructions are not undisputed in any of the language families involved, we may never know in each case which languages borrowed from which others, or whether any may share the form as a cognate from a common parent language.

4.11. *The second apple term*

Friedrich's (1970:60-62) second IE "apple" form, which is disputed, is attested in the following:

Doric Greek	μᾶλον "fruit, apple" (poetic "cheek")
Homeric Greek	μῆλον "fruit, apple"
Latin	mālum "apple, fruit"
Albanian	mollë "apple (apple-cheeks)" (possibly a loan from Latin)
Hittite	maḫla- "grapevine, apple(-tree)" ?
Tocharian A	malañ "cheeks" (metaphorical shift ?)
Cf. Hittite	mu-ri-eš "grape"

The probable relation of "apples" to "grapes" and to "fruit" in IE raises other interesting possible connections. In this respect, we find several U forms which offer suggestive similarities. In what follows, I list these with some discussion where helpful.

(1) "Berry" 1.

PU ***murå*, PFU **murå*, PS **mərå* "berry" (Sammallahti 1988:538); PU ***mora* "raspberry, cloudberry" (Collinder 1960:407); PU **mura* "Rhubus chamaeomorus" (Décsy 1990:103); PU ***mura* "Rhubus chamaeomorus" (Rédei 1986-1988:287).

Finnish	muurain, muurama "cloudberry, arctic raspberry"
Zyrian	mi̮r- "cloudberry, arctic raspberry"
Vogul	morax "cloudberry, arctic raspberry"
Ostyak	mūrə̑x "cloudberry, arctic raspberry"
Yurak Samoyed	maraŋa "cloudberry, arctic raspberry"
Yenisei Samoyed	moragga "cloudberry, arctic raspberry" (SKES 355)

The Uralic cognate set bears a strong similarity to Latin *morum*, Greek μόρον "mulberry", which Friedrich (1970:150) links as possible cognates worthy of further study. Both these and the "apple" terms are similar to the next U set.[14]

(2) "Berry" 2.

PFU **marja* "berry" (Collinder 1960:412; cf. Rédei 1986-1988:264):

Finnish	marja "berry"
Mordvin	mal "apple" (Moksha), "berry" (Erza)
Cheremis	mör, -mə̀·rə "berry"
Vogul	mo̮ari, morip "berry cluster"
Ostyak	mūr̮ə̑p', mūrəp "berry bunch" (SKES 334)

The nature of the relationship between these last two cognate sets is unclear — they appear to be distinct, yet to have a certain overlap.

(4) "Berry" 3.

PFU **med'i* "berry" (Sammallahti 1988:544); PFU **mel'ɜ* "cherry" (Collinder 1960:412); PU ***matɜ* "a berry species" (Rédei 1986-1988:265), PFU **mol'ɜ* "a berry of a certain shrub" (Rédei 1986-1988:279):

Zyrian	mol' "berry"
Ostyak	*mel "berry"
Votyak	mul'i "berry"
Hungarian	mëggy "cherry"

4.12. *Beech*

For "beech", Friedrich (1970:106) reconstructs PIE **bhāgo-*; Watkins (1985:5) gives **bhāgo-*. However, Indo-Europeanists have proposed reconstructions with five different vocalisms (Friedrich 1970:106). Some of the IE cognates are:

Mod. Icelandic	baukr "mug, box" beyki "beech forest" (probably invalid cognates)
OHG and MHG	buohha "beech woods"
OHG, Old Norse	bōk "beech" (*ō* < PIE **ā*)

Mod. English	beech
MHG	butchen "to wash or soak in hot lye or buck"
Old English	būk "buckets"
	(Old English and MHG are not real cognates)
Thraco-Illyrian	mūgo, mūso "beech"
Lydian	mūsós "beech"
	moesia "beech forest"
Old Ch. Slavic	bъzъ "elder"
Russian	buziná "elder"
Ukrainian	byže "elder"
Albanian	bungë "Quercus esculentis"
Serbo-Croatian	bâz "elder"
Czech	bez "elder"
	bzína "elderberries"
Latin	fāgus "beech"
Celtic	bago "beech" (the Celtic form is complicated because in some of the languages the word has been borrowed from Latin, leaving the status in the others uncertain)
Doric Greek	φᾱγός "Quercus esculentis"
Attic Greek	φηγός "Quercus esculentis" (with edible acorns)

Beeches do not grow in the Baltic region nor in most of the FU area. This accounts for why Baltic languages have no reflexes of **bhāgo-*, and perhaps for why the Slavic languages have shifted the meaning to "elder".

Though the beech is not found in most of the FU area (Friedrich 1970:113-114), its importance for "nuts" and the semantic shifts to "elder(berries)" and "oaks (with edible acorns)" suggest a possible connection with FU terms for "nuts":

PFP **päški* "nut" (Sammallahti 1988:553); PFP **päškɜ* "nut, hazelnut" (Rédei 1986-1988:726)

Finnish	pähkinä "nut" (dialectal "acorn")
Mordvin	p′ešt′e, p′eštše "nut" (*št* < **šk*)
Cheremis	pükš "nut"
Votyak	paš-pu "nut bush" (*-pu* = "tree")
	paš-mul′i "nut" (*-mul′i* = "berry")
	(SKES 679)

Although the FU cognates extend this term only to Proto-Finno-Permic times, it may be older, with no surviving cognates in the other languages.[15]

Another cognate set, which is phonetically similar but semantically not so equivalent, can also be compared with the IE "beech" forms. It is the "rowan" (mountain ash), which also produces berries. Sammallahti (1988:553) reconstructs it only to Proto-Finno-Permic, but SKES (542) and Collinder (1960:413) finds it throughout FU:

PFU **pićla* "mountain ash, rowan" (Sorbus aucuparia) (Rédei 1986-1988:376); PFP **piśla* "rowan (mountain ash)" (Sammallahti 1988:553); PFU **pićlä* "rowan" (Collinder 1960:413)

Finnish	pihlaja "rowan"
Mordvin	p′iźol "rowan, rowan berry"
Cheremis	pəzə·lmə, pi·zlə "rowan, rowan berry"
Zyrian	pelyś "rowan berry"
Votyak	paleś "rowan berry", paleś-pu "rowan"
Ostyak	pəĎa`r, pəźà`r "rowan berry"
Vogul	pit′šə̀·r, pə̄šer "rowan berry"
Hungarian (?)	fagyal "honeysuckle"
	(Sammallahti 1988:553; SKES 542)

Räsänen (1955:27; cf. also SKES 542) also pointed out similarities in some Turkic languages, with the possibility of borrowing from FU into Turkic:

Chuvash	pilеš "rowan"
Kazan Tatar	miläš "rowan"
Turkish	meleš "rowan"
Tobol	micär "rowan" (this Tobol form was believed to be a loan from Vogul, cf. Räsänen 1955:27)

4.13. *Yew*

Friedrich (1970:121) reconstructs IE **eywo-* "yew", although it means "yew" consistently only in Germanic and Celtic; it is based on the cognates:

Greek	ὀā, οἴη "service tree, bird-cherry"
Latin	ūva "grape, bunch of grapes, laurel berry"
Armenian (?)	aigi, oigi "grapevine"
Lithuanian	ievà "black alder"
Latvian	iva "bird-cherry"
Old Prussian	iuwis "yew"
Czech	jiva "yew"
Serbo-Croatian	ìva "willow" (the majority forms mean "willow")
OHG	īwa (German *Eibe*) "yew"
Old Norse	ýr "bow"
Breton	ivin "yew"
Gaulish	ivo- "yew"
Middle Welsh	yw-en "yew"
Old Irish	ēó, í "yew" (< **iwo-*)
	ibhar "bow, yew"

The U forms for "yew" are strikingly similar:

PU ***joxɨ* "tree", PFU **juxi*, PS **jë*, PUg **jugi* "tree" (Sammallahti 1988:537); PU ***juwɜ* "yew" (Collinder 1960:406); PU ***juwe* "Pinus silvestris"

(Hajdú 1979:37); PU **juva* "Pinus silvestris" (Décsy 1990:99); PU ** *juwɜ* "Pinus silvestris" (Rédei 1986-1988:107)

Finnish	juko(-puu) "yew" (*-puu* = "tree")
Estonian	juka(-puu), juha(-puu) "yew"
Cheremis	jakte "high-trunked tree, pine, spruce, birch"
Ostyak	jux "tree, wood"
Vogul	jiw "tree, wood"
Proto-Samoyed	*je, *jew "pine" (Janhunen 1977:42) (Hajdú 1964:54; Collinder 1960: 406; SKES 122)[16]

4.14. *Oak*

There are several terms for oaks in IE. Friedrich (1970:140) points out that cognates for **dorw-* "tree, oak" are attested in all the stocks (**derw-*, **drw-* also).[17] Some of the cognates are:

Hittite	taru "wood"
Sanskrit	dắru "piece of wood, wood, wooden implement" drumá- "tree" (in later texts) druṇam "bow"
Avestan	draos "of the wood" drvō 'hale"
Modern Persian	daruna "rainbow"
Greek	δόρυ "beam, wood, spear" δρυ(Ϝ)άς "wood nymph" δρῦς, δρυός "oak (?), tree"
Latin	larix (?) "larch" dūrus "hard"
Lithuanian	darvà "tar, log, piece of pine wood" drútas "strong, stout"
Latvian	darva "tar"
Old Ch. Slavic	drěvo "tree"
Russian	dérevo "tree"
Armenian	targal "spoon" tram "firm, fixed"
Old English	trēo(w) "tree"
Albanian	dru, druni "beam, piece of wood, post, tree" dry, dryni "wooden peg"
Old Irish	drochta "(wooden) trough" derucc "acorn" doar "oak"
Cornish	dar "oaks"
Middle Welsh	derw-en "oak"

As Hajdú (1964:57) has pointed out, there are no "Quercus" forms which extend to FU times. Only if the oaks had a wider distribution in FU times would we expect terms for them in the proto-language. Nevertheless, there are some terms in FU languages which bear remarkable similarities to this IE etymon. These are presented in the following examples:

(1) "Acorn": Hakulinen (1968:254) finds this term as far back as Proto-Finno-Permic or Proto-Finno-Volgic times, though this is unconfirmed:

Finnish	terho "acorn"
Vote	turu, toro "acorn"
Estonian	tõru, toro "acorn"
Livonian	tę'rmə̂z "acorn" (SKES 1273)

Given the limited distribution among FU languages and the striking phonetic (and semantic) similarity to IE forms, one would suspect borrowing from IE.

(2) "Fir, pine": Another possibility is:

PFP **tirkä* "fir" (Sammallahti 1988:550)

Cheremis	terke "pine" (Hajdú 1964:56)
Vogul	täreg "fir"
Ostyak	teeger "fir"

(3) "Oak": Hajdú (1964:56) has given a possible, but doubtful FU cognate set for "oak":

Hungarian	tölgy "oak" (< **til-*)
Ostyak	tal "forest"
Zyrian	tel' "wood of a *Nagelbaum*"

4.15. *Another "oak"*

Among his miscellaneous tree terms which warrant further study, Friedrich (1970:150) lists the following additional "oak" term:

Proto-Germanic	*danwō ("oak" in one 11th-century gloss, but generally "pine" in OHG)
Old Ch. Slavic	dǫbъ "oak"
Sanskrit	dhánu-ḥ "bow" dhanvana-ḥ "fruit tree"

One etymon found among Finno-Ugric languages for terms for "oak" bears a similarity to these IE forms:

PFV, PFP (?) *toma "oak" (Rédei 1986-1988:798)

Finnish	tammi "oak"
Mordvin (Erza)	tumo "oak"
Mordvin (Moksha)	tumă "oak"
Cheremis	tum, tù·mə̂ "oak" (SKES 1218)
Votyak	ti-pi, tə-pə (*-pi*, *-pə* = "tree")
Zyrian	tu-pu (*-pu* = "tree") (Rédei 1986-1988:798)

A different, more extensively attested U etymon can also be compared to this set of IE forms:

PU ***d'ɨxmɨ*, PFU **d'ɨɨmi*, PS **jëθm* "bird-cherry" (Sammallahti 1988:536); PU **dhjoma* "Prunus padus" (Décsy 1990:99); PFU **δ'ōme* "bird-cherry" (SKES 1408); PU ***δ'ęme*, ***δ'ōme* "bird-cherry" (Rédei 1986-1988:65)

Finnish	tuomi "bird-cherry"
Proto-Lapp	*δōmę (Lehtiranta 1989:32)
Mordvin (Moksha)	lajńe
Mordvin (Erza)	l'om
Cheremis	lompâ, lombo (< *lom* + *pu* "tree")
Votyak	l'ęm
Zyrian	l'ęm
Vogul	l'ēm, l'ām
Ostyak Samoyed	t'eu, t'šem "bird-cherry" (SKES 1408)[18]

4.16. *Linden*

For IE trees connected with "linden", Friedrich (1970:88) considered **lenTā*, **lēipā*, *llwyfen*. As pointed out by Friedrich (1970:89), the linden was both technologically and ritually important to both IE and FU speakers. Nevertheless, the IE status of the tree names is not clear. Some compared forms include:

Greek	ἀλίφαλος· "a species of oak" (?)
Celtic	Lemo-, Limo- (in place names)
Middle Welsh	llwyfen "elm, linden"
Balto-Slavic	*lēipā "linden"
Lithuanian	líepa "linden"
Latvian	liẽpa "linden"
Common Slavic	*lipa "linden"
OHG	linta "linden"
Icelandic	lind "linden"
English	linden
Lithuanian	lentà "board"
Albanian (Tosk)	lëndë "wood, material"

These can be compared with two different U etyma:

PFV **lemšɜ*, **leme-šɜ* "young linden" (Rédei 1986-1988:688)

Finnish	lehmus "linden"
Mordvin	l'evš "linden bast"
Cheremis	nemeštə "young linden" (Rédei 1986-1988:688)

And:

PFV **leppä* "alder" (Rédei 1986-1988:689)

Finnish	leppä "alder, birch"
Lapp	lei'pē, liahp "alder"
Mordvin	l'epe, l'epä "alder" (Rédei 1986-1988:689)

4.17. *Hornbeam ?*

Friedrich (1970:99-106) considers IE cognates which support his reconstruction **grōbh-* "hornbeam" (genus *Carpinus*), of which he says, the wood is heavy, hard, and elastic, but is "ideal for tools, weapons, and armor" (Friedrich 1970:99). Terms for "hornbeam" were used for "beech" in some IE dialects (Friedrich 1970:101). Some of the forms cited by Friedrich are:

Old Prussian	wosigrabis "Carpinus betulus"
Lithuanian	skrúoblas "hornbeam"
Latvian	Gruõblas (a place name)
Ukrainian	hrab "common hornbeam" (*Carpinus betulus*)
Greek	γράβ- "a type of oak"

These can be compared to FU forms with which they bear a certain (though not overly compelling) similarity:

PFU **kappɜ* "a species of tree which can be worked: poplar, aspen, fir"

Votyak	kipi "log, block"
Ostyak	xap "aspen, boat"
Vogul	gap- "fir, poplar" (Rédei 1986-1988:12)

4.18. *Some loans*

I finish this paper by briefly mentioning some cases of known loans in FU from IE involving terms for trees.

(1) The Balto-Finnic word for "tar" is a loan involving the IE **dorw-* "oak, tree" etymon. It is believed to be borrowed either from Baltic (Lithuanian *darvà* "tar", *dervà* "pitch, tar, resinous pine tree"; Latvian *darva* "tar") or from Germanic **terwōn* "tar" (SKES 1276):

Finnish	terva "tar"
Estonian	tõrv "tar"
Livonian	tera "tar"

(2) Another fairly clear loan involves other "oak" forms. In this case, it also shows how semantic shifts can be involved in the transfer of tree names. Borrowed from Proto-Scandinavian **aik-* "oak" (cf. IE **ayg-* "oak" [Friedrich 1970:132]) are:

Finnish	aihki (dialectal *aikki*) "tall pine, tall spruce"
Proto-Lapp	*ājkke "ancient tree"
Luleå Lapp	hai'hka "large pine"
Norwegian Lapp	hai'kâ, ai'kâ "large pine, a tree which grows in the south" ("oak"

only in fairy tales)

(3) Another set of loans involves the religious connotations associated with IE **perk^{w}-* "oak" (Friedrich 1970:133). As Friedrich points out, this oak term had various religious associations; cognates in Indic refer to "rain cloud, god of storm, paradise tree, sacred fig tree, mountain". Slavic tribes had *perunъ* "the thunder god", while Celtic and Baltic had similar deities. Germanic forms mean "forest, mountain, tree, man, thunder god, and mother of Thor". Finnish has the borrowing *perkele* "devil, satan" (obscene, used for cursing) from Baltic; cf. Lithuanian *perkúnas*, Latvian *pērkuônis* "thunder, thunder god". Other Balto-Finnic languages have related forms, e.g., Vote *perku* "hell", Estonian *pergel*, *põrgel* "devil", *põrgu* "hell". Erza Mordvin *Puŕǵińe* "thunder" is also borrowed from Baltic. Finnish *piru* "devil, satan" (also an obscene curse word) may be from Old Russian *perunъ* "thunder god" (cf. SKES 523-552, 576-577; Friedrich 1970:113-140).

(4) Friedrich (1970:55) reconstructs a second IE "willow" term, **wyt-*, based on the cognates:

Russian	vit' "woven object"
	vetla "willow"
Lithuanian	vytìs "willow switch"
Latin	vītis "willow, grape, tendril, shoot"
Greek	εἰτέ(ϝ)ᾱ "willow, willow shield"
	οἶσος "willow species"
Irish	feith "fiber, cord"
Old English	wīðig "willow"
Old Norse	víþir "willow"
Sanskrit	vetasa-ḥ "willow, rod, switch"
Avestan	vaētay "willow, switch"
Armenian	(?) gi "juniper"

In all nine IE stocks, the terms refer not only to "willow", but also to "willow withes" or "shoots" and to artifacts made from them (Friedrich 1970:54). These can be compared to the Balto-Finnic terms:

Finnish	vitsa "switch, rod"
Vote	vittsa
Estonian	vits "twig, switch"
Livonian	vitsà
	(SKES 1799)

These and the IE terms appear to be related in some way. SKES (1799) tentatively attributes the Finnic forms to borrowing from Russian; Hakulinen (1968:254), on the other hand, gives these a more native origin, finding cognates for them that go back to Finno-Volgic or Finno-Permic times. SKES (1977) does not, however, believe the Latvian forms *vica*, *vice* "switch" to be native, attributing them to borrowing from either Estonian or Russian. Friedrich (personal communication, 1973) holds it more likely that the Finnic forms are loans from Slavic.

5. CONCLUSION

In summary, the two language families, FU (or U) and IE, share a rather large number of similarities among their names for trees. While conceivably some of these compared forms are but fortuitously similar, the weight of the aggregate of comparisons is sufficient to support the conclusion that these two language families have a very old historical connection, one which reflects either a genetic affiliation or Sprachbund affinities, or perhaps both. The connection may involve diffusion, and indeed, certain of the forms presented here almost certainly involve borrowing. Given this set of circumstances, the possible areal linguistic relationship to explain these and other observed similarities shared by languages of these two families deserves serious consideration. On the other hand, at this stage we cannot rule out the possibility that the similarities among some of the tree names explored here may perhaps reflect an old genetic relationship — a common ancestor. Further study ought to keep both hypotheses open, and it is hoped that the comparisons presented here will indeed stimulate further research aimed at determining the exact nature of the historical connection shared by IE and FU (or U).

NOTES

1) While some may be accidental similarities, the nature and quantity of the aggregate argue strongly that chance is not the explanation for most of them.

The abbreviations used in this paper are:

FU	Finno-Ugric
IE	Indo-European
MHG	Middle High German
OHG	Old High German
PFP	Proto-Finno-Permic
PFV	Proto-Finno-Volgic
PFU	Proto-Finno-Ugric
PIE	Proto-Indo-European
PS	Proto-Samoyed
PU	Proto-Uralic
PUg	Proto-Ugric
SKES	*Suomen Kielen Etymologinen Sanakirja* (Toivonen et al. 1955-1978)
U	Uralic

2) Broader proposals for Uralic are at best controversial. For example, Uralic is often listed in the literature with Altaic, in the so-called Ural-Altaic hypothesis. This, however, is not supported, and, in fact, many of the leading "Altaicists" have recently dismantled Altaic as a group, showing that the evidence available does not support the genetic affinity of several of the languages usually identified as "Altaic" (cf. Unger 1990). Other proposed far-flung connections which, at least for the present, lack full support are: Uralo-Dravidian, Eskimo-Uralic, Uralo-Sumerian, etc.

3) My principal source for IE is Friedrich (1970), who was the major stimulus for my research on this topic. I have also consulted Pokorny (1959) and Watkins (1985).

Unless otherwise stated, citations and discussions of IE matters follow Friedrich. My principal sources for Uralic (U) and Finno-Ugric (FU) are: Toivonen et al.'s (1955-1978) etymological dictionary of the Finnish language, henceforth SKES; Collinder (1960, 1965); Hajdú (1965, 1969, 1975a); Janhunen (1977); Rédei (1986-1988); and Sammallahti (1988); plus others cited less frequently. Examples are as found in the sources, using their orthographic symbols. Although these are not always consistent with one another, they are sufficiently clear for the purposes of this paper.

4) According to current thinking about dating Uralic language splits, it is believed that Proto-Uralic separated into Proto-Finno-Ugric and Proto-Samoyed ca. 6,000 - 4,000 B.C. Proto-Finno-Ugric split into Finno-Permian and Ugric ca. 4,000 - 2,000 B.C. (T. Itkonen 1984:350 holds ca. 2,500 B.C. to be the latest date before the end of Proto-Finno-Ugric unity). There is no reliable dating for the end of the Finno-Permian period, but Proto-Finno-Volgic is calculated to have split at the latest ca. 1,500 B.C., and Early Finno-Baltic at the latest ca. 1,000 B.C. (Korhonen 1984:66; T. Itkonen 1984:350).

5) The honeybee was unknown in Siberia, Turkestan, Central Asia, Mongolia, and most of the rest of Asia, but was found in eastern Europe west of the Urals with a northern border coinciding with that of the oak.

Another diffused word that has given rise to much speculation is "metal", with cognates in nearly all Uralic languages meaning "copper, iron, ore, or metal" (e.g., Finnish *vaski* "copper, bronze, brass"; Mordvin *víśke* "metal, wire"; Cheremis *βa* "ore, metal"; Zyrian *-iš* "metal"; Votyak *-veś* "metal"; Ostyak *wax* "metal, iron"; Hungarian *vas* "iron"; Yurak *wieśśə* "iron, money"; Yenisei *bése* "iron"). Since Uralic dates to the Stone Age, such an ancient term for metal is significant. Possibly, this stone-age population came to know copper in the Urals, where it is found on the earth's surface (Korhonen 1984; Hajdú 1975a:35). Nevertheless, it is important to keep in mind that a metal term of similar shape is found widely also in Indo-European languages, as well as in Sumerian, so that it may be a very old *Wanderwort* (Joki 1973:339-340).

6) The widely-held notion that the homeland was in the region of the Middle Volga stems from Aminoff (1873). On the other hand, according to another also widely-held view, the homeland would have been further east and perhaps further north, between the Urals and the Volga-Kama-Petšora area or on both sides of the Ural Mountains (Paasonen 1923; Zsirai 1937; Sebestyén 1952; Hajdú 1969, 1975a, 1975b). Supporters of a third view believe that the Proto-Finno-Ugric population, at least in its final phases, may have occupied a rather wide area from the Urals to the Baltic, based on the notion that hunting and fishing groups need wide territories (Ojansuu 1907; E. Itkonen 1966; cf. Sammallahti [1984:153] and Hajdú [1975a:37]).

In an argument from negative information, it has been suggested that a *terminus ante quem* for Finno-Ugric can be established on the basis of tree names. "Oak" has a different name in Finno-Permian languages (e.g., Finnish *tammi*) from Hungarian (*tölgy*). Hajdú (1975a:41) takes this to mean that by the time the "oak" arrived to the Petšora region (3,000 - 2,000 B.C.), the Finno-Permian languages had already separated from the Ugric branch.

7) Note that references given at the end of a list of cognates refer to the sources of the entire cognate set. However, occasionally forms cited from some other source are inserted into these lists, in which case the references appear immediately adjacent to these inserted additional forms.

8) Räsänen (1955:28) reported similar forms in other languages in the general region, e.g.:

Turkish (M.)	kajŋ "Scots pine"
Chuvash	xirə "Scots pine"
Soyot	xady "Scots pine"

9) Similarities from other languages of the region include:

Tungus	sē-kta "Salix viminalis"
Uighur (*Turkish)	sögüt "willow" (Räsänen 1955:27)

10) Compare the phonetically similar but distinct PFU **peća* "willow" (Rédei 1986-1988:367):

Votyak	puč'i "bud, willow catkin"
Zyrian	paća
Hungarian	füz "willow"

Compare also forms from other languages:

Tungus	fodo "willow"
Korean	padil "willow" (Räsänen 1955:27)

11) Watkins (1985:47) presents the possibility that the Latin form is from **peį̯ə-* "to be fat, to swell" with a suffixed zero-grade, **pī-nu-* ("pine tree, yielding a resin").

12) Sinor (1969:279) has pointed out some similarities in other languages of the area:

Yakut	tīt "pine"
Altaic Turkic dialect	tït "larch"

Räsänen (1955:26) compared also:

Tungus	sakso-kumar "conifer"
Goldic	sese "Larix dahurica"

13) Hajdú (1969:257) considered another possible connection between IE and U in citing the conifer terms:

IE	*kuse "spruce (Picea)"
PU	**kuse "spruce (Picea)"

I do not find the IE term confirmed elsewhere, however. The U form is based on one of the most solid of U cognate sets.

14) For additional evidence of areal diffusion, compare Tungus *moriŋo* "cloudberry", which Räsänen (1955:28) thought to be perhaps borrowed from Samoyed, and

Tobol (Tatar) *myrak* "cloudberry", which Räsänen (1955:28) believed possibly to be a loan from Vogul.

15) Conceivably one could also compare the IE forms to another etymon:

PFV	*päkšnä "linden"
Estonian	pähn "old linden"
Mordvin	pekše, päšä "linden"
Cheremis	pitsə, pištə "linden"
	(Rédei 1986-1988:726)

16) Similar forms from a different cognate set have sometimes erroneously been attributed to this set. Nevertheless, it is interesting to note the similarity of forms representative of PU ***jakkɜ* "pine or spruce forest" (Rédei 1986-1988:107) with the compared IE forms.

17) Watkins (1985:12) reconstructs **deru* "tree, wood", with primary senses "to be firm, solid, steadfast". He gives a variant form **derw-*, in Germanic **terw-*, which has the Old English cognate *te(o)ru* "resin, pitch".

18) Compare also:

Turkish	jumurt "bird-cherry"
Sagai	namyrt "bird-cherry"
Mongolian	žimu-gu-sun "bird-cherry"
	(Räsänen 1955:27)

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REPORT FROM THE FIELD: THE TOCHARIANS?

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This past summer while in Siberia, I had the opportunity to talk to Sergei Grigorevich Skobelev, chief archaeologist of the Laboratory of Humanitarian Research, Novosibirsk State University. He works primarily in the period of the Iron Age and is particularly interested in the Tashchtik culture (2nd c. BC - 5th c. AD) and its predecessor, the Tagar culture (7th c. - 2nd c. BC). These cultures existed along the southern border of Western Siberia neighboring Northern Kazakhstan. The central area of the cultures extended from Krasnoyarsk City south to Abakan on the Yenisei River and along it to Sayanogorsk. Dr. Skobelev's work touches on the archaeological identification of the Tocharians, an especially difficult problem. Below are a few notes from two conversations I had with him which may be of interest to those interested in the cultural aspects of the Tocharians. (Also, see J. P. Mallory, *In Search of the Indo-Europeans*, 1989.)

The last period of the Tagar culture is related to the Tashchtik culture, and occupied an area in the southern part of the later Tashchtik culture along the Yenisei above Sayanogorsk. Skobelev is quite convinced that these Tashchtik people are the Tocharians and bases this conclusion on the archaeological data he has discovered.

At the end of the Tagar and beginning of the Tashchtik period, the kurgans, which mark the graves of these people, are larger than those of the preceding Bronze Age. They frequently contain many large stones on the inside and out, and sometimes multiple burials. One such kurgan with 200 skeletons is a mass grave with people "just tossed in." The Tagar people have many similarities to the Scythians, while the Tashchtik are physically a mixture of Scythian types and Mongols. In the Tashchtik graves, there are many gypsum death masks — almost all of men — both Mongolian and European types but mostly Mongolian. At the end of the Tashchtik period, men were frequently cremated but the women who were with them were not. This may relate to different cultural backgrounds for the men and women or perhaps just a gender difference in burial ritual, in light of the fact that women are buried with poor goods. The usual burial is one man with 4 sacrificed women . Typically, there was a hole in the back of the female's skull where the brain had been removed. Skobelev and his colleagues are unable to tell if the hole was the cause of death or occurred immediately after death. The women were European and the men Mongoloid. A similar situation has been found in Pazyryk graves which are found just south of the Tashchtik area. (For a full account of the Pazyryk graves, see Rudenko, Sergei L., *Frozen Tombs of Siberia: The Pazyryk Burials of Iron-Age Horsemen*, 1970.) Only a few women were buried with great wealth and these also had masks. There is clear influence from China because of the presence of ivory hair pins.

The Huns, who were Turkic speakers, followed the Tashchtik in this area, and it was the Huns who brought iron to these people. Hun graves can be distinguished from Tashchtik on several accounts. For example, many Hun graves had horses but none are found in Tashchtik or Tagar graves. Iron horse bits came with Huns. Bows and arrows are found in female as well as male Hun graves. Women skeletons also exhibit battle wounds, perhaps confirming the statements of the 4th c. Roman historian Ammianus Marcellinus, who wrote about Hun women in battle with Germans. Hun men wore earrings only in left ear. Stelae have often been found on kurgans similar to those found on IE kurgans — some carved, some not. There can be several to a single grave. Pictures of people, animals, horses, axes, houses, and daily life appear on both the stelae as well as on the stone walls of the graves. Huns mixed with Europeans, but they were mostly Mongoloid.

It may, therefore, be that the Huns drove out the Tashchtik people south to the Tarim basin. This basin is located directly south of the area mentioned above and it is the Tarim basin which is the historical location of the Tocharians. The question is: how far back can we take this? Can these people, the Tashchtik, be linked to the Afanasievo culture, which dates back to the third millennium BC and whose remains have similarities to the Yamnaya culture of the Pontic-Caspian area?

For those who may be interested in this problem, contact Sergei Grigorevich Skobelev, Chief Archaeologist of the Laboratory of Humanitarian Research, Novosibirsk State University, Pirogova, 2, 630090 Novosibirsk-90, Russia.

ON THE GENETIC CLASSIFICATION OF BASQUE

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During the past few decades, the topic of the genetic classification of the Basque language, when not avoided entirely, has divided scholars into two main camps: those favoring (1) the Afroasiatic origin or (2) the Caucasian[1] (or Dene-Caucasian) origin of Basque. To the first school belong Hugo Schuchart, G. von der Gabelentz, and, until recently, the late Hans Mukarovsky. To the second belong C. C. Uhlenbeck, Alfredo Trombetti, René Lafon, Karl Bouda, Georges Dumézil, and, more recently, Vjačeslav Čirikba, Václav Blažek, and the present writer. In a recent issue of this journal, it was reported that A. B. Dolgopolsky had joined the Basque-Afroasiatic school, and it was stated that "the Basque-Berber connection is

a serious competitor to the proposed link of Basque to Caucasic" (McCall and Fleming 1994:26). With respected scholars and dozens of lexical comparisons on both sides, how do we decide?

I propose that we carefully examine the evidence for both proposals in terms of the two major criteria for genetic classification: (1) similarity in core (basic) lexemes and (2) similarity in grammatical formatives. For criterion (1), it is necessary to focus on the core (or basic) lexicon of the languages involved: the semantic fields least likely to erode over millennia of time. For that purpose, the comparisons listed here fall within Dolgopolsky's (1964) hierarchy of the twenty-five most stable meanings.[2]

1. (a) Basque ***ni*** "I" (ergative ***nik***), compared by Mukarovsky with Berber *nək*, *nəkki* "I", Egyptian *ỉnk*, Akkadian *anāku*, etc.; (b) Basque ***ni*** "I": cf. Caucasian **nɨ* (Lak *na*, Dargi *nu* id.).
2. (a) Basque ***gu*** "we", compared by Mukarovsky with Berber *nəkni* "we"; (b) Basque ***gu*** "we": cf. Caucasian **ƺ[ü]* "we" (inclusive: Avar *n-iɬ*, Andi *iɬi*, Agul *ɣi-n*, Tabasaran *uxu*).
3. Basque ***bi*** "two": cf. Caucasian **q'wi̲* (Ubykh *t'-q'ʷa*, Archi *q̲'ʷe*, Udi *p̄a̲*). (Basque *bi* from **ɢʷi*, influenced by *bat* "one", cf. English *four*, *five*; Latin *quattuor*, *quinque*).
4. (a) Basque ***hi*** "thou" (2nd person singular), compared by Mukarovsky with Berber *kiyyi* "thou"; (b) Basque ***hi*** "thou": cf. Caucasian **ɢu* (Taskhur *ẏu*, Dargi *ħu*, Chechen *ħo*, *aħ*); Burushaski *gu-* "thou".
5. Basque ***zu*** [*s̄u*] "you" (2nd person plural): cf. Caucasian **ɣwV* (Lak *zu*, Chechen *šu*, Kabardian *fe*, etc.).
6. Basque ***ze-r*** "what?", ***ze-in*** "which?": cf. Caucasian **sa* (Ubykh *sa* "what?", Dargi *se*, etc.); Burushaski *be-s*, *be-sa-n* "what?".
7. Basque ***no-r*** "who?", ***no-iz*** "when?": cf. Caucasian **nV* (Agul *na*, Lezgi *ni* "who?", Tsezi *ne-ti* "when?"); Burushaski (Werchikwar) *ána*, *ánɛ* "where, whither?".
8. (a) Basque ***mihi*** "tongue", compared by Mukarovsky with Semitic: Amharic *məla·s* "tongue", Argobba *mäla·s*; Chadic: Boghom *ŋəlis*; (b) Basque ***mihi*** "tongue": cf. Caucasian **me̲lc'i* "tongue" (Circassian *bzə*, Andi *miᾱ'i*, Tabasaran *melz*, etc.); Burushaski *melč̣* "jaw" (metonymy).
9. (a) Basque *i-**zen***, (Vizcayan) *u-**zen*** "name", compared by Mukarovsky with Berber *isem*, *isəm*, (Zenaga) *isem*; Arabic *'ism-* (and other cognates in Semitic, Cushitic, Chadic, Omotic); (b) Basque *i-**zen***, (Vizcayan) *u-**zen*** "name": cf. Burushaski *sen-as* "to say, to tell, to call, to name", *sen-as* "named". The Basque alternation of prefixes suggests that the word may be archaic rather than borrowed. Cf., more remotely, Na-Dene: Tlingit *sa*, "name, voice", *-sa*, *-sen* "to name, to breathe".
10. (a) Basque ***begi*** "eye", compared by Mukarovsky with Egyptian *bɜ̓k* "to see", and forms like the following in Cushitic: Oromo *be·ku·* "to know"; Omotic: Bencho *bek'a* id.; (b) Basque ***begi*** from **beryi* "eye":[3] cf. Caucasian **ʔwilʕi* "eye" (Tabasaran *ul*, Hunzib *hare*, Khinalug *pil*, Avar *ber*, Chechen *bʕärg*, etc.); Burushaski *-l-či(n)* "eye (of needle)", *-i·l-* "eye" (in compounds).
11. Basque ***bihotz*** (*bi-**hotz***) "heart": cf. Burushaski *-as* "heart" (from **has*). (For the Basque fossilized class prefix *be-/bi-*, cf. also §§15, 32, below.)
12. (a) Basque ***hortz*** "tooth", compared by Mukarovsky with Berber (Rif) *a-qarruš* "tooth"; (b) Basque ***hortz*** "tooth": cf., by metathesis **ħɨl̲cV* ~ Caucasian **cɨl̲ħV* "tooth" (Circassian *c̄a*, Dargi *cula*, Chechen *cer-g*, etc.); Burushaski *-a-waš* (*-me*), (Werchikwar) *-hašɛ* (*-mɛ*) "molar (tooth)"; (c) Basque ***hortz*** "tooth": cf. Caucasian **gwəlƺwē* "fang, canine tooth" (Lak *k̄arc̄i* "tooth", Avar *gožó* "fang, canine, etc.").
13. Basque ***ez***, (Vizcayan) ***ze*** "not": cf. Caucasian **ćə/*ć'ə* "not" (Chechen *ca*, *-c*, Ingush *cə-*, Batsbi *co-*); Burushaski (Werchikwar) *ačho* "not yet".
14. Basque ***oskol*** (Vizcayan) "nail, claw", compared by Mukarovsky with Berber *isker* "nail". (The Basque word has many dialectal variants: *ezkazal*, *azazkal*, *azüskülü*, etc., some with the meaning "hoof").
15. Basque *be-**hatz*** (Labourdin) "nail": cf. Caucasian **kwač'e* "paw" (Avar *kʷač'*, Dargi *kač'a*, etc.).
16. Basque ***nigar***, ***negar*** "tears, weeping": cf. Caucasian **newq'u* (Dargi *nerẏ* "tear", Lak *maq'* id., Chechen *not'q'a* "pus"); Burushaski *nagei* ~ *magei* "boil" (sore with pus).
17. (a) Basque ***mama*** "water, potable liquid" (child speech), compared by Mukarovsky with Berber *ama-n* "water", Arabic *ma·'-un*, etc.; (b) Basque ***mama*** "water, potable liquid": cf. Burushaski *mamu* "milk, sap (of crops)".
18. Basque ***hur*** "water" (usual word): cf. Caucasian **ħ̲wirɨ* "lake" (Avar *ħor*, Lezgi *wir* ~ *ür*, etc.); Burushaski *hur-o·ɣo* "sweat", *hur-iginas* "stream, current, wave".
19. Basque *u-**hin*** (Labourdin) "wave (in the water)": cf. Caucasian **xä̲nɦɨ* "water" (Chechen *Xi*, Avar *ɬim*, Dargi **xin*, etc.).
20. (a) Basque ***hil*** "to die, to kill; dead", *herio* "death", compared by Mukarovsky with Semitic *h r g* "to kill", Cushitic: Khamir *qɨru·*, etc.; (b) Basque ***hil*** "to die, to kill; dead": cf. Caucasian **-i(w)ƛ'V* "to die, to kill" (Circassian *ƛ'a-n*, Chechen *-al-* id., Hunzib *-iƛ'-* "to kill").
21. Basque ***bartz*** "nit": cf. Caucasian **nɛ̲mƺe* "louse" (Abkhaz *a-ć'a*, Lak *nac'*, Chechen *meza*, etc.).[4]
22. (a) Basque ***hil-*** "moon", compared by Mukarovsky with Berber (Tuareg) *ta-lli-t* "new moon"; Arabic *hill-un* "appearance of the new moon (Monatsanfang)"; (b) Basque ***hil-***, ***hil-argi***, ***il-az-ki*** (Basse Navarre) "moon"; cf. Burushaski *halanc* ~ *halanz* "moon".
23. Basque *hil-**argi***, ***argi**-zagi* "moon"; cf. Caucasian **(wi̲-)rəq'A* "sun" (Ubykh *n-dẏa*, Lezgi *raẏ*, Lak *barẏ*, etc.).
24. Basque ***esku*** "hand": cf. Burushaski *hi·sk* "wrist, back of the hand", (Werchikwar) *hɛsk* "base of the palm of the

hand and thumb".

25. (a) Basque ***bortz***, ***bost*** "five" (from "hand"), compared by Mukarovsky with Berber *a-fus* "hand"; (b) Basque ***bortz***, ***bost*** "five" (from "hand"), *bost-eko* (Guipuzcoan, Vizcayan) "either one of the hands (cualquiera de las manos)": cf. Caucasian **bo̲rcV* (Tsezi *besi* "fist", Tabasaran *bac* "paw", Avar *púrc̄i* "ham, [animal's] gammon"); Burushaski *bac-in* "thigh".
26. Basque *u-**khare*** "wrist" (*u-**khal-*** in compounds): cf. Caucasian **q'w[ä̲]l̲ʔV* (Ubykh *q'ā-p'á* "hand", Avar *q̄'ʷal* "arm, sleeve", Khwarshi *q'e* "shoulder").
27. Basque *a-**hur*** "palm (of the hand)": cf. Caucasian **kwīlʔɨ* "hand" (Archi, Udi *kul*, Avar *kʷer*, Hunzib *koro*, etc.). Basque *h* ~ Caucasian **k(w)* is regular (cf. §15, above).
28. Basque ***gau*** "night" (*gab-* in compounds): cf. Caucasian: Lezgian **X̲am* (Taskhur *X̲am* "night", Tabasaran *X̲ab-as* "evening"); Abkhaz *-Xa* "night".
29. (a) Basque *a-**dar*** "branch, horn", compared by Mukarovsky with Berber (Tuareg) *de·le* "branch (Zweig)", cf. also Coptic *tar* "point, end, branch"; (b) Basque *a-**dar*** "branch, horn": cf. Caucasian **ƛ̄wɨ̲rV* (Avar *ƛ̄ar* "horn", Chechen *kur* "[wild animal's] horn"); Burushaski *tur* (unprefixed), *-ltur* (with prefix) "horn".[5]
30. Basque ***bethe*** "full": cf. Caucasian **bVtV* (Kabardian *bəda* "strong, solid", Chechen *butu* "solid, dense", etc.); Burushaski *bu·ṭ* "much, many, numerous, very".
31. Basque ***oso*** "whole, entire, healthy": cf. Caucasian **fioc'V* "full, to fill" (Ubykh *-za*, Lak *-u-c'u*, Tabasaran *ac'u*, etc.).
32. Basque *be-**larri*** "ear" (Vizcayan, Guipuzcoan): cf. Caucasian **l̲eHl̲e* "ear" (Abkhaz *-lə-mħa*, Dargi *liħi*, Chechen *ler-g*). Basque *be-**harri***, *be-**arri*** in other dialects probably was influenced by the verb *beha-* "to listen, to look".

DISCUSSION OF LEXICAL COMPARISONS

Taking a raw "score" on the comparisons, without any evaluation, we have 13 Basque-Afroasiatic comparisons, 26 Basque-Caucasian comparisons, and 17 Basque-Burushaski comparisons. On the face of it, all these scores could strongly suggest genetic affinity of the languages in question, but the narrow range of scores (13 ~ 26) would not be considered decisive by many linguists. Let us take a closer look at the comparisons:

(1a) Basque *ni* ~ Berber *nək(ki)*: Basque ergative *ni-k*, but *-k* is a distinct morpheme that is suffixed to nouns as well, so the comparison is really *ni* with Afroasiatic **ʔ n k* or the like (derived by Blažek from **ʔan-aku*). The Caucasian **nɨ* is much more likely.

(2a) Basque *gu* ~ Berber *nəkni*: Once again, comparison with wider Afroasiatic (e.g., Semitic **naḥna/u*) indicates two nasals, neither of which is found in Basque. Again, comparison with Caucasian **ƛ̣[ü]* is more straightforward. **ƛ̣* is a lateral fricative, and the close relationship between laterals and velars is abundantly evident both in Basque and Caucasian languages: e.g., Caucasian **rä̲ƛ̄'ɨ* "meat, flesh" (with glottalized lateral affricate = *t̲ɬ'*) becomes Andi *riƛ̄'i*, but Lak *dik'*, Khinalug *likˉa*, with velars like Basque *a-ragi* "meat, flesh".

(8a) Basque *mihi* ~ Amharic *məla·s*, etc.: wider comparison in Afroasiatic shows that the root is really **lis*, and the *m-* is apparently a prefix (or, the words come from the root **l ḥ s* "to lick).

(8b) The phonetic development in Basque may have been **milčˉi* > **mišˉi* > **mixi* > *mihi*. The first stage, with retroflex *-lčˉ-*, is preserved in Burushaski. The development of *š* or retroflex *ṣ* to a velar fricative (*x*, *xʷ*) is widely attested (e.g., in Castilian Spanish, Swedish, Slavic). Finally, there is good reason to believe that earlier **x* is one of the sources of modern Basque *h* (cf. §33, below). A parallel case could be Basque *behi* "cow" < **bexi* < **beṣi* < **berčˉi*, cf. Caucasian **bħ̲rc'wV* (Avar *bóc̄'i* "cattle").

(10) Basque *begi* ~ Egyptian *bꜣḳ*, etc.: the meaning is verbal rather than anatomical. The Afroasiatic words do not account for the *r* in Souletin *ber-*, while the Dene-Caucasian explanation does.

(12) Basque *hortz* ~ Berber *a-qarruš* "tooth": If affinity with broader Afroasiatic is sought, the usual word for "tooth" (**s n*) is quite different. I prefer the solution of (12b): the form **ħɨ̲l̲cV* (eliminated in Caucasian proper by the metathetic **cɨ̲l̲ħV*) corresponds perfectly with Basque *hortz* and Burushaski **-hʷaš-* (see the correspondences established by Bengtson 1991c).

(17, 18) The form *mama* "water" is associated with infantile language (and universal Lallwörter), while the usual word *hur* is common Dene-Caucasian (cf. also Yeniseian **xur* "water").

(22, 25, 29) Again, the Dene-Caucasian explanations are semantically and/or phonetically more straightforward than the Afroasiatic. Note the precise morphological correspondence of the Basse Navarre and Burushaski words for "moon": *il-az-ki* ~ *hal-anc* (22b).

With these evaluations taken into account, the case for genetic affinity of Basque with Caucasian and Burushaski becomes much more decisive. Barely a handful of Basque-Afroasiatic comparisons now seem plausible, while the Basque-Caucasian and Basque-Burushaski[6] parallels are both more numerous and more plausible, semantically and phonetically, than the Afroasiatic alternatives. Most important, Schuchardt, Mukarovsky, and other scholars have found no

parallels between Basque and Afroasiatic *interrogatives* and *negatives*, though parallels in these fields are found between Basque, Caucasian, and Burushaski (§§6, 7, 13). I conclude from this that Basque is closest to Caucasian and Burushaski, while Afroasiatic, if related at all, must be more distant. This is the same conclusion Trombetti, Lafon, and others arrived at several decades ago.

There remain, however, three fairly acceptable Basque-Afroasiatic comparisons in the above list: (4a) Basque *hi* "thou" ~ Berber *kiyyi*, (9a) Basque *izen* "name" ~ Berber *isəm*, and (14) Basque *oskol* "nail" ~ Berber *isker*. How are these to be explained, in the face of a likely Dene-Caucasian affiliation of Basque? In my opinion, (4a) could represent evidence of a deeper relationship between Dene-Caucasian and Afroasiatic (cf. Starostin 1989), while (9a) and (14) more likely represent loanwords (if we discount comparison [9b]) by Basque from Berber or a Berber-like language. Centuries of contact with the Tartessians, on the far southwest corner of Iberia (Spain-Portugal), may have been the source of such loanwords (cf. McCall and Fleming 1994:25).

A notable fact about Basque-Berber parallels is that, while the presumed genetic cognates are phonetically and/or semantically difficult, cultural or technological words are very similar, viz., Basque *bide* "road" ~ Berber *a-brid, ta-brida*; Basque *izten* "awl" ~ Berber *t-isten-t* (*t* is the feminine marker); Basque *matel* "cheek" ~ Berber *a-ma·ḍəl* (words denoting parts of the face, unlike most other anatomical words, are not uncommonly borrowed, e.g., English *face, jaw* from French). This tells us that most of the similarities between Basque and Berber are due to historical contact rather than genetic origin.

There are in fact several layers in the Basque lexicon. To give just a few examples: *Kartvelian*[7] (Basque *khako, krako* "hook" ~ Georgian *k'ak'vi* id.; Basque *larre* "pastureland" ~ Svan *lare* "meadow"; Basque *apho* ~ *aphu* "toad" ~ Svan *apXw* "frog"); *Egyptian* (Basque *zazpi* "seven" ~ Coptic *sašf, šašf* "seven"; Basque *berri* "new" ~ Coptic *bēri, brre* "new, young"; Basque *arrain* "fish" ~ Coptic *rame, raame, rami* "fish"); *Berber* (as noted above; cf. also Basque *zilh-ar* "silver" ~ Basque *a-zerf*; Basque *urhe* "gold" ~ Berber *uriɣ* "yellow" ~ or Laz *orXo*, Georgian *oXro* "gold"[8]); and finally *Latin* (Basque *luma* "feather" ~ Latin *pluma* "feather"; Basque *bilho* "hair" ~ Latin *pilu-* "hair" are among the few Latin words that have penetrated into Basque). But all this has affected the basic vocabulary of Basque very little. The great majority of meanings on the Swadesh 100-word list can be accounted for by Caucasian or Dene-Caucasian. Just a few examples will be given here:

33. Basque ***txori*** [*čori*] ~ ***xori*** [*šori*] "bird": cf. Caucasian **č'wɻlV* (Avar *č'orólo* "quail", Chamalal *č'or* "bird", Lak *č'il-mu* "small bird"); Burushaski *čili* (*babu·k*) "a very small bird"; Sino-Tibetan: Tibetan *m-čhil-pa* "sparrow" (Blažek and Bengtson 1995, §58).
34. Basque ***hotz*** "cold": cf. Caucasian: Avar *kʷač-* "coolness, frost"; Yeniseian: Pumpokol *ki·č-idin* "cold"; Na-Dene: Athabaskan **k'azʔ* "(to be) cold" (Hupa *k'ac'*, Navajo *-k'àz, -k'às*) (Blažek and Bengtson 1995, §185).
35. Basque ***su*** "fire" ~ Caucasian **c'ajɨ* "fire" (Avar, Dargi *c'a*, Lak *c'u*, Chechen *c'e*, etc.); Burushaski *ši* "fireplace", *šu-tin* "hearthstones", (Werchikwar) *šu-tum* "hearth, stone" (Bengtson 1990, §25).
36. Proto-Basque **a-xʷo* "mouth" (***aho, ago, abo, ao*** in the various dialects): cf. Sino-Tibetan **Kho(w)H* "mouth" (Dimasa *khu*, Old Chinese **khu*); Yeniseian **Xowe* "mouth" (Ket *qo·*, Assan *xoboj*); Na-Dene **Xu·ʔ* "tooth" (Eyak *Xu·-*, Kutchin *(á)-ɣwo*) (Blažek and Bengtson 1995, §5).
37. Basque *i-**z**ar* "star": cf. Caucasian **Ha-ʒwarʕi* "star": Abkhaz *-jac'ʷa*, Akhwakh *c̄'ʷari*, Avar *c̄'ʷa*, etc. (Čirikba 1985:102, §84).

Let us now go on to the other criterion of genetic classification, which is grammatical affinity. As we have already seen, pronominal parallels between Basque and Afroasiatic are scanty at best, and agreements between the respective interrogatives and negatives are nonexistent. Such common Afroasiatic features as the *t* feminine and internal *a*-plurals are totally unknown in Basque.

But positive evidence, not negative, is decisive. Numerous grammatical parallels between and among Basque, Caucasian, and Burushaski have been detailed by Trombetti, Lafon, Dumézil, and others (see, e.g., Čirikba 1985; Bengtson 1990, 1991d, 1991e, 1991f, 1993). Among these are the fossilized noun prefixes *a-, e-/i-, o-/u-, be-/bi-* of Basque, which have been compared with the class systems of Caucasian and Burushaski. (Examples of the prefixes can be found in this study: §§9, 11, 15, 19, 26, 27, 29, 32, 36, 37):

a-hur "palm", ***a-dar*** "horn", ***a-ho*** "mouth";
i-zen "name", ***i-zar*** "star";
u-hin "wave", ***u-khare*** "wrist";
bi-hotz "heart", ***be-hatz*** "nail", ***be-larri*** "ear".

The inflectional (case) endings of Basque also have counterparts in the same languages (Bengtson 1993). It turns our that the grammatical comparison is even more decisive than the lexical, and that here too Basque is clearly closer to Dene-Caucasian (especially Caucasian and Burushaski) than to Afroasiatic.

Basque is an "unimportant" language, in terms of numbers of speakers or political or economic importance. But, for the prehistory of language, the value of Basque is inestimable, as the sole remnant, west of the Caucasus, of a once far more widespread family of languages: Dene-Caucasian. The various layers of the Basque vocabulary tell a fascinating story, which is yet to be fully told. René Lafon (1949:202) distinguishes the Basque physical type ("ce type paraît se rattacher à la race paléolithique de Cro-Magnon") from the Basque language ("apparentée aux langues caucasiques"). "C'est une langue d'origine étrangère" (1949:206), adopted by the ancestors of the Basques, just as their counterparts in western Iberia and Gaul adopted Celtic, and later Latin, along with corresponding cultural and social

changes.

NOTES

1) For most linguists in the first half of this century, "Caucasian" or "Caucasic" included Kartvelian ("South Caucasian") as well as Northwest Caucasian and Northeast Caucasian. In recent years, there has been a growing consensus that Kartvelian is genetically quite distinct from (North) Caucasian. (Linguists of the Nostratic School generally include Kartvelian in Nostratic but include [North] Caucasian in Dene-Caucasian.) Since the designation "South Caucasian" has been cast aside in favor of "Kartvelian", "North" Caucasian is now simply "Caucasian".

2) Because this is not a glottochronological calculation, comparisons with plausible semantic shifts are included. (The intention is to focus on basic semantic fields). Basque-Afroasiatic comparisons are taken from Mukarovsky (1969, 1981), and Basque-Caucasian and Basque-Burushaski comparisons are drawn from Čirikba (1985), Blažek (1991 [=1992]), Blažek and Bengtson (1995), and Bengtson (1990; 1991a, 1991b, 1991c, 1991d, 1991e, 1991f; 1993). Because I consider Basque to belong to a subgroup (Macro-Caucasian) of Dene-Caucasian, most of the comparisons here involve the three entities: Basque, Caucasian, and Burushaski. (More remote branches of Dene-Caucasian are cited in a few cases.) Caucasian reconstructions and attested forms follow Nikolaev and Starostin (1991, 1992). Unless indicated otherwise, Basque forms are cited from more phonetically conservative "French" dialects rather than the *h*-less "Spanish" dialects: e.g., *mihi*, *hortz* rather than *mi(i)*, *ortz*.

3) Basque **beryi*, which recapitulates the Avar and Chechen forms, is based on the Souletin form *ber-phuru* "eyebrow", from **bert-buru* from **berɣ-buru*, according to Basque rules of combination; cf. Burushaski *-l-puʻr* "eyelash", a compound of the same two morphemes.

4) Both Basque and Caucasian can be derived from a form such as **welʒe* > **wenʒe* > **menʒe* > **nemʒe*, by a series of assimilations and dissimilations. Remote comparison with Na-Dene (Tlingit *wés'* "louse") confirms the antiquity of the first form in the series (**welʒ-* or **welc'-*).

5) Burushaski *t-*, *-lt-* are the regular correspondences to Caucasian *ǟ-*, *-ǟ-*. The Basque form was probably originally **a-rdar* or **a-ldar*, simplified to *a-dar*, perhaps influenced by Egyptian *tar* (§28a).

6) The higher score of Caucasian as opposed to Burushaski can be explained by two factors: (a) the large number of Caucasian languages, and (b) the relatively scant amount of Burushaski material available to linguists.

7) That is, Basque parallels with Kartvelian. The direction of loans is not always clear. Most probably, they result from a period when the linguistic ancestors of the Basques lived in geographic proximity to the Kartvelians (in the area now occupied by the Ossetes?).

8) French *or*, Spanish *oro* "gold", etc. (from **auso-*) are true "chance resemblances"!

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LETTER FROM JERRY KING TO HAL FLEMING

O si yu
Greetings: ᎣᏏᎶ

Hal, thanks for everything and your continued interest — a letter to follow when I'm feeling better. Quickly did these two notes. Thought you might find this interesting, as it represents salvage work. Did this while recovering from surgery. Had to use what was near. Pardon my shaky hand and scratch paper, but wanted to get ideas down. Don't know if this is what you want for Mother Tongue or not, but thought you might like to see the notes as it fills a gap in Americas. More when feeling better.

ᏤᏗ *To hi*
"peace and health".

Sincerely,
Jerry ᎤᏂᎵᏍᎪ

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A NOTE ON OFO *skálo* "HEAD"

JERRY KING
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I had been studying the Ofo language, a Siouan language of the Southeastern U.S.A. for a long time, when I happened to meet the late Claude Medford, Jr., a Choctaw, in 1988. Claude had had the good fortune to know very well Alice Picote (Tatoo, her name in Ofo), the granddaughter of Rosa Pierrette (Little Flint, her name in Ofo), the informant of J. R. Swanton, who saved the Ofo language for the scientific world. Claude became intimate friends of Alice and made a pot in the shape of a human head with tatoo marks at Alice's request to be buried with her on her death. He and Chief Joseph of the Tunica buried her, honoring her request. Claude gathered much additional information on the little documented southeastern tribe, and gladly we exchanged our information, which led to some important discoveries.

Greenberg (1987:172) correctly shows two competing forms for "head" in his subgroup: Almosan-Keresiouan of Northern Amerind. Ofo seems to have preserved cognates of both forms. Most of the Siouan languages have cognates of the Proto-Siouan word for "head" from **pa*, indeed the most common words for "head" in Ofo are descended from this Proto-Siouan word. They are *apha*, *-pha*, and *-pa*. Greenberg does not list these Ofo cognates. However, Ofo seems to have preserved a cognate from the second proposed term for "head" which in Ofo is *skálo* (Dorsey and Swanton 1912:329). It seems to share this cognate with a closely related language Tutelo, which, together with Biloxi, make up what has been termed Ohio Valley Siouan by Voegelin (1941:246-249) and Southeastern Siouan by Haas (1968:84). The Tutelo cognate listed by Greenberg is *sako* = "above". Also, another Siouan language, Catawba, appears to have a cognate which is listed by Greenberg as *-skã* = "head". All of these in turn appear to be related to Cherokee, listed by Greenberg *u-sko-li*, *-sk-* = "head". For those interested in deeper relation, Greenberg considers Wakasan Kwakiutl *səqʔa* = "over" and Nisqualli, a Mosan Salish language, *šuk*, *šišuk* = "above" as cognates as well.

Claude remarked that Alice referred to the U. S. currency quarter as "double head" because of the eagle head on one side and Washington's head on the other. Fortunately, Swanton gathered from Alice's grandmother Rosa the term for quarter as *skálo nápha* = "two head". Whether Rosa fully explained this to Swanton or not I do not know as I have not seen his field notes. At any rate, Swanton gave the etymology of *skálo* as a translation of French *escalin*, the French coin valued at 12½ cents or, in English, a "bit". He implies that it is a borrowing from French. But note that he *does not say* it is a *borrowing*. Nor have I found where he did. Both Claude and I rejected the idea that it was a borrowing and firmly believed it to be a true cognate with Cherokee and Catawba. We both believed it to be a "bit" (pardon the pun) of folk etymology here.

There are several other reasons for rejecting it as a borrowing. Alice also referred to the U. S. currency gold coin, commonly referred to in English as the "double eagle" as "double head". This certainly suggests a much wider usage than the French *escalin* or 12½ cents.

Further, the name "double head" was and is a popular personal name among southeastern Native Americans. This could easily account for its persistence in Ofo as an archaic term. "Double head" or *tal tsúska*, *tar tsúska* is a very common Cherokee personal name, and the name of a very famous Cherokee Chief. The name has deep metaphysical meanings. It is also a common art motif, deeply rooted in Native American art from early Adena times (ca. 1,000 BC) and attested in art forms since then by numerous artifacts. One further note here, one still sees in print that this design was borrowed from the European royal coats-of-arms, especially the Hapsburgs. This is simply not the case, as it is attested for over 3,000 years in eastern North America and perhaps the northwest U.S.A. as well.

Since there is a scarcity of Ofo material and the difficulty of using what has been preserved, the discovery of a cognate *skálo* = "head" in Ofo to other Almosan-Keresiouan terms should aid us in better understanding the evolution of the speech of Native America as well as the evolution of the speech of the human family.

Northern Amerind

A. Almosan-Keresiouan
   1. Almosan
      (a) Algic

(b) Kutenai
(c) Mosan
(1) Chemakuan
(2) Salish:
Nisqualli: *šuk, šišuk* "above"
(3) Wakasan:
Kwakiutl: *səqʔa* "over"

2. Keresiouan
(a) Caddoan
(b) Iroquoian:
Cherokee: *u-sko-li, -sk-* "head"
askoli, uska, -sk- "head"
(c) Keresan
(d) Siouan-Yuchi
(1) Yuchi
(2) Siouan:
Catawba: *-skã* "head"; *sako, sag-* "above"
iskã "head"
Ofo: *skálo* "head" (archaic form competing with *-pha-, -pa-* [the more common form])
Tutelo: *sako* "above"

B. Penutian
C. Hokan

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A NOTE ON CATAWBA *Weyaline* "CHIEF'S TOWN"

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It is well known in scientific circles and the general public as well that the greatest linguistic diversity in aboriginal North America was found in the California-Oregon area, as far as we presently know. Dr. Mary Haas made this point in 1970 (1971:44) and pointed out the reason was that his area, especially the northern part, was largely little affected by the inroads of European civilization until the middle of the nineteenth century.

At that same time, Dr. Haas also pointed out to scientific circles that there was another area of great linguistic diversity in the native Americans and that it was certainly in the southeast U.S.A. and the adjacent coast of the Gulf of Mexico. A fact well known among knowledgeable Native Americans. She pointed out that we may never know just how diverse this area really was because of the sparseness of information, especially so for the critical period before Native Americans had been seriously dislocated by the pressures of competing European nations. It is conceivable that it just may have been more diverse, possibly the most diverse in aboriginal North America. Because of the lack of data, however, we may never know. As she pointed out, "many smaller tribes in the Southeast [U.S.A.] have almost certainly vanished without a trace. Others are known to us by names only..." (Haas 1971:44). And I should like to point out that, even among well documented languages of the Southeastern United States, there are serious gaps in documented dialectal variations and archaic forms, as many of which have not survived into modern times. These gaps are sometimes a serious handicap in reconstructing accurate proto-words and, therefore, seriously handicap comparative work. These problems are not only of concern to Americanists but others, especially "long rangers". So it behooves us to use every means possible to aid in overcoming these serious problems. It is to these concerns that I address these notes.

Recently, I was shown a photocopy of a map dated 1756 of the Cuttahbaw Nation (the modern Catawba Nation of South Carolina, U.S.A.). They have recently been in the news media as they have settled a land claim and reestablished a federal relationship (reservation, etc.). The map shows several Catawba towns, and a number of place names can be recovered from it. One of the towns is labeled "Weyane or the King's Town". The name of the town is repeated two more times on the map, labeling "trails" to the town itself and both times spelled "Weyanne". I have found the name of the town again in John Evans Journal to the Catawba Nation. Here it is as "Weyaline". Again with these words "the Catawbaw King...his town" (McDowell 1970:107). We thus have three attested forms, *Weyaline* from the text and *Weyane* and *Weyanne* from the map. And we are told it translates as "Chief's Town" or "King's Town" (i.e., "Kingston") in English. Immediately, one knowledgeable of Catawba will recognize that the first syllable *we* does indeed mean "town". This then leaves us with the syllables *-yaline, -yanne, -yane*, which we are told mean "chief" or "king". Here we have a problem as in modern Catawba the word for "chief" is *yę swą'* and *migráehę* which does not match, at least in modern Catawba (Speck and Schaeffer 1942:564). Thus, it would appear "Chief's Town" is not accurate, or is it? Here, comparison is of value. Catawba is a Siouan language, eastern division. In Ofo, another Siouan language, western division, the word for "chief" is *itcólĕ* (< *iyólĕ* in Archaic Ofo). Here, we have a possible match, especially with the archaic form. If Ofo *iyólĕ* and Catawba *-yaline* are cognates, then *Weyaline* does probably mean "Kingston", and *-yaline* is an archaic form in Catawba.

Is there any supporting evidence to strengthen this argument? Again, we turn to old documents. The *South Carolina Gazette*, published on June 2 and on June 7, 1746, call the Catawba King *Yanabe Yalengway*. Note the syllables in the second word *Yalen-* and note also he is referred to as "The

Catawba King" (Brown 1966:224). If *yaline* in the Evans text means "king" in 1756 and variations *yane* and *yanne* appear in the same year of 1756 on the map with the meaning "king", and we now have ten years earlier in 1746 in the South Carolina newspapers *yalen*, a part of the *Catawba King's Name* and a likely cognate in Old Ofo *iyólĕ* meaning "chief", then it is very probable that *yaline* is an old Archaic Catawba word meaning "king, chief, leader" based on a root other than the modern forms recorded.

One other note: I collected the Catawba term */yurena/* which my Catawba informant glossed as "my lead" (i.e., "leader") in reference to a black and tan hound which was the "lead dog" of his hunting pack of "coon dogs". He called his favorite "lead dog" by the Catawba name */wadesɟ/* or "old gourd" from *wade* = "gourd" and *sɟ* = "old" because of the dog's long nose and the black and tan color. Now compare his reference to the dog as *yurena* = "my lead (dog)" based on *yure* = "lead" and *-na* = "my". This suggests a very good translation of *weyaline* as *we* = "town", *yali* = "leader", *ne* = "my". If Evans had a translator or interpreter, which he stated he did, then this translator would have probably responded to Evans inquiry with the Catawba term *weyaline* or "my leader's (i.e., chief or king) town". This certainly compares nicely with *yurena*, "my leader".

My present thinking is that we have recovered a Catawba term derived from an Archaic Catawba term which fell into disuse as a political term in modern terms and offers us an opportunity to project back 4,000 to 6,000 years ago to Proto-Siouan times and a new piece to the puzzle of what Proto-Siouan contained with the reconstructed Proto-Siouan term **yale* = "to lead; leader".

This, then, permits us to look at an even deeper connection of 6,000 to 8,000 years ago to Macro-Proto-Siouan times. For the Proto-Siouan term **yale* = "to lead; leader" may ultimately be connected at least to the **ya* syllable, to my own Cherokee suffix *-ya* found in the tribal name of the Cherokee *Yvwiya* from *Yvwi* = "human being" and *-ya* = "principal, original, etc." We now have a Macro-Proto-Siouan term **ya* meaning "lead, principal, original, etc."

In these notes, I have used numerous extra-linguistic materials , such as old maps, journals, newspapers, place names, field notes of survivors, etc., as well as linguistic data. In this search, no possible lead must be overlooked. Research of this nature *must*, to be fruitful. I grant that much work needs to be done: the exact phonetics involved, more examples, especially of archaic forms and dialectal variations, but we *must have* the data to examine, if we are to proceed in unveiling the past and ultimate origins of our speech.

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ALTAIC, GERMANO-EUROPEAN, AND NOSTRATIC: THE EVIDENCE OF PHONETICS AND PHONOLOGICAL SYSTEMS

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1. Phonetics, Phonological Systems, and Nostratic

The bulk of the argumentation for or against the Nostratic theory has centered on shared lexical items which appear to have corresponding relationships among the sounds, particularly among the occlusive obstruents. Thus, for example, Kaiser and Shevoroshkin (1988 – following Dolgopolsky 1964) note the correspondence between Indo-European *k^{w}er- 'to build' and Altaic **Kurv-* 'build', in which both the obstruents and the trills correspond. In other instances, correspondences appear to have been affected by intervening sound laws, as in Indo-European *g^{w}en- 'woman, wife' and Altaic (Turkic) **küni* 'one of the wives (in polygamy)'.

One aspect of the correspondences that has received little attention, however, is how the corresponding obstruents operate within their systems. It is not enough to establish correspondences between obstruents in isolation, for the "same" obstruents may or may not correspond, depending upon the systems in which they occur.

1.1. Borrowing between Systems

When languages (or more properly, dialects) come into contact with one another, there is usually some degree of lexical borrowing. When a word in one language is borrowed into the other, though it is pronounced with the "accent" of the borrowing language and it is thus incorporated into the borrowing lexicon with the phonetic characteristics of the borrowing language and within that language's own phonological system (compare Fries and Pike 1949).

Occasionally, a sound is borrowed from one language into the other in order to accommodate the lexical item being loaned. For example, it has been demonstrated in Griffen 1974 that the affricates were borrowed from English into Welsh to render certain English words. Once they were incorporated within Welsh, however, they ceased to be treated as English sounds and adhered to the phonological system of Welsh.

The development of the Welsh affricates has taken place on two levels. On the phonological level, the tenuis

affricate [č] now alternates with the media affricate [ǰ] in the mutation system just as the tenuis stop [t] alternates with the media [d]. Moreover, a series of nasal affricates [n̥h/ň] has developed to accommodate nasal mutation in the dialect of Dyffryn Nantlle (see also Jones 1971). Thus, the incorporation of the affricate into the Welsh phonological system is complete only when it functions within the morphonological alternation patterns of the language.

It is on the phonetic level, however, that the initial incorporation must take place. While the consonants of English are differentiated by voice, those of Welsh adhere to a four-member aspirate fortis-lenis scale. It would be quite impossible for the affricates as English sounds within the English system to participate in the mutation system based upon the fortis-lenis scale. In borrowing the tactic of affrication, Welsh thus alters not its system, but rather the perception of affrication from that of a voicing system to that of an aspirate fortis-lenis system.

In borrowing, the phonetic underpinnings of a phonological system do not change just because a foreign word is incorporated into the lexicon. Indeed, as demonstrated in the phonemic categorization experiments (see especially Scholes 1968), no matter how well the speaker knows the foreign language, the speaker perceives the sounds only within the system of the native language.

For example, speakers of Swabian, an Alemannic dialect in south-western Germany, have long had contact with French, at least through the nearby Alemannic dialect of Alsatian. Nonetheless, the French word *pardon* [pardõn] has been borrowed as [bardõ] by the Swabians (see Kauffmann 1980:176, Griffen 1983:151-52). The tense voiceless unaspirate labial stop within the French obstruent system based upon muscle tension is interpreted as the lax voiced unaspirated labial stop within the Swabian obstruent system based upon fortis aspiration. While the Swabian [p^h] would have actually been closer to the French [p] in its phonetic features, the number of corresponding features is not what is important – what is important is the correspondence of (un)aspiration alone, for that is the pertinent opposition in the borrowing system.

1.2 Sound Correspondences in Nostratic

Within the comparative method, Nostraticists have relied upon sound correspondences between words in the various language groups, as pointed out in section 1. Thus, Indo-European *k^wer- 'to build' is seen to be related with Altaic **Kurv*- 'build', partly because the dorsals in both correspond with one another, and the lexical items are close enough in meaning to have been derived from a common source. Moreover, such researchers as Kaiser and Shevoroshkin (1988) as well as Bomhard (1992 – see also Bomhard and Kerns ms.) demonstrate that the corresponding sounds can be related by sound laws, just as are the corresponding sounds among the various Indo-European (or Germano-European) language groups.

Critics of the Nostratic theory point out that such correspondences and the sound laws governing them may well be the result of borrowing. For example, the fact that the consonants in German *Straße* [ʃtra:sə] 'street' can be shown to correspond through regular sound laws with those of Italian *strada* [stra:da] 'street' does not demonstrate a common ancestor at all. German simply borrowed the word from Latin at a time prior to a relatively late sound shift.

Certainly, it is difficult to determine whether sound correspondences among languages at the (pre-)historical "depth" of the Nostratic language families may result from borrowing or from genetic relationship. Thus, Campbell (1990:174-75) concludes his study of similarities between tree names in Indo-European and Finno-Ugric (or Uralic) as follows:

> In summary, the two language families, FU (or U) and IE share a rather large number of similarities among their names for trees. While conceivably some of these compared forms are but fortuitously similar, the weight of the aggregate of comparisons is sufficient to support the conclusion that these two language families have a very old historical connection, one which reflects either a genetic affiliation or Sprachbund affinities, or perhaps both. The connection may involve diffusion, and indeed, certain of the forms presented here almost certainly involve borrowing. Given this set of circumstances, the possible areal linguistic relationship to explain these and other observed similarities among some of the tree names explored here may perhaps reflect an old genetic relationship – a common ancestor. Further study ought to keep both hypotheses open, and it is hoped that the comparisons presented here will indeed stimulate further research aimed at determining the exact nature of the historical connection shared by IE and FU (or U).

Thus, it is just as easy for proponents of the Nostratic theory to see sound correspondences among the pertinent families as evidence of genetic relationship as it is for opponents of the theory to see the same data as evidence of borrowing. A greatly neglected fact about borrowing though (see above and Griffen 1974), is that the sounds would have to have been incorporated within the borrowing phonological system based upon the phonetic characteristics of the borrowing language. In no event could the phonetic basis and its phonological system have been borrowed.

The question now for historical linguists, whether they support or oppose the Nostratic theory, is this: Are the phonological systems of the Nostratic cluster of language families and their phonetic bases close enough to one another to suggest a genetic relationship? Given the many possibilities for sound systems available to unrelated language families, the occurrence of regular sound correspondences between similar phonetic/phonological systems would make a strong case for the genetic relationship. Conversely, the occurrence of regular sound correspondences between significantly different phonetic/phonological systems would make a strong case for borrowing.

This article will provide a starting point for the answer to this question. With the new reconstruction of Germano-European (Griffen 1988, 1993) rather than the traditional Indo-

European, the relationship between this family and Nostratic will be examined (as in Griffen 1989) to see how closely the phonological systems and their apparent phonetic bases correspond. Next, the obstruent system of Altaic will be examined to see if there is indeed an argument for systematic correspondence between at least these two hypothesized Nostratic families.

2. Germano-European and Nostratic

German-European (See Griffen 1988) is a term applied to a reconstruction of the proto-language consonant system for the language groups heretofore known as Indo-European. While the Indo-European reconstructions have been carried out in the old segmental phonetic/phonemic tradition, the Germano-European reconstruction has been accomplished through the more modern dynamic analysis (see Griffen 1985). Based upon a more reliable system of analysis without the prejudices of segmental letters, this reconstruction maintains that the development of the proto-language obstruent system was rather simple in Germanic, but indicative of a more complicated shift in the "Indo-European" (non-Germanic) language groups.

2.1 Phonetic Plausibility

Moreover, the reconstruction inhances its reliability through the theory of phonetic plausibility (Griffen 1993). An outgrowth of Prague School phonology with its insights into the way feature/oppositions are related through the phonologically pertinent opposition (see especially Trubetzkoy 1969:76-77, Jakobson 1962:23), this theory maintains that predictions and reconstructions of change must adhere to the functioning of the system and to the pattern of change inherent to it. For example, in a lenitive environment (word-final, intervocalic, etc.) in an aspirate fortis-lenis system with [t] as fortis and [d] as lenis, a lenitive change (from [t] to [d]) is plausible, while a provective change (from [d] to [t]) is implausible. Since the consonants among the language groups correspond along the fortis-lenis scale, the provective Grimm's Law is implausible, while the Germano-European reconstruction is plausible, for it adheres in all instances to known fortis-lenis relationships.

The Germano-European reconstruction is based upon the phonetically plausible developments in an aspirate fortis-lenis system. In such a system, a fortis-lenis scale such as that characterized by [ð] - [d] - [t] - [θ] from lenis to fortis is based upon the phonetic parameter of fortis aspiration, characterized by an increasing ratio in high-to-low frequency emissions from lenis to fortis (see Griffen 1975a:chapter 10, 1985:chapters 5 and 7, 1993). As seen in historically attested changes in Welsh, this type of system can support the following expectations for change (all quite plausible, but listed in descending order of plausibility):

1. Where accent is not a factor, intervocalic consonants may change to the lenis.
2. Where accent creates a position of fortis strength,

Table 1: Germano-European Reconstruction

	Reconstruction	Germanic	Indo-European
*p^{h}-	p^{h}ĕt- 'foot'	Got fōtus 'foot' (3)	Lat pedis 'foot (gen.)'
*-p^{h}-	nep^{h}od- 'grandchild, nephew'	OIc nefe 'nephew' (3)	OP napāt- 'grandchild'
*p-	paita- 'goat skin'	Got paida 'garment'	Grk βαίτη 'goat(skin) garment' (1G)
*-p-	deu-p- 'deep'	Got diups 'deep'	Ill bythós 'deep' (1)
*-p-	upér 'over'	OHG ubir 'over' (1)	Av upara 'the upper' (2/1)
*b-	ber- 'to carry'	Got bairan 'to carry'	Skt bhárati 'carries' (1G)
*-b-	terb- 'to wind'	OHG zerben 'to turn'	Skt dṛbháti 'winds' (1)
*t^{h}-	t^{h}rei- 'three'	Got þreis 'three' (3)	OIr tri 'three'
*-t^{h}-	u̯et^{h}- 'year'	Got wiþrus 'yearling' (3)	Hit witt- 'year'
*t-	tu̯o(u) 'two'	Got twai 'two'	Alb dü 'two' (1G)
*-t-	set- 'to sit'	Got sitan 'to sit'	OCS sĕděti 'to sit' (1)
*-t-	k^{h}m̥tóm 'hundred'	Got hund 'hundred' (1)	Lat centum 'hundred' (2/1)
*d-	dĕ(i)- 'to nurse'	Got daddjan 'to nurse'	Skt dhāya-ḥ 'nourishing' (1G)
*-d-	beud- 'to observe'	OS biodan 'to present'	Skt bodhár 'knower' (1)
*k^{h}-	k^{h}u̯on- 'dog'	Got hunds 'dog' (3)	Toc ku 'dog'
*-k^{h}-	t^{h}ak^{h}- 'to hush'	Got þahan 'to hush' (3)	Umb taçez 'silent'
*k-	kăr- 'to call, to cry out'	Got kara 'care'	Grk γῆρυς 'voice' (1G)
*-k-	sāk- 'to track'	Got sōkjan 'to seek'	OIr saigim 'to seek' (1)
*-k-	sĕk- 'to cut'	Got sega 'saw' (1)	OCS sekyra 'ax' (2/1)
*g-	ger- 'to jut forth'	OIc grǫn 'mustache'	Grk χαρήν 'upper lance point' (1G)
*-g-	steig- `to climb'	Got steigan 'to climb'	Skt stighnoti 'climbs' (1)

intervocalic consonants either do not change, or change to the fortis.

3. A consonant in a position of strength (as in word-initial position) may change to the fortis. (Griffen 1988:174, also 1989:143)

From this phonological system with its aspirate phonetic basis, the reconstruction of Germano-European can be accomplished as in table 1 (p. 40), in which the numbers refer to the tendencies noted above and (G) to a generalization of a tendency to a new environment. All reconstructions are thus phonetically plausible in keeping with the phonetic basis of the phonological system.

The original Germano-European obstruents thus maintained three members: (1) a strongly articulated aspirata *t^h, (2) a less strongly articulated (less aspirate) tenuis *t, and (3) a more weaky articulated (relatively least aspirate) media *d. From this reconstruction, the developments in all the families are well within expectations, as an aspirata may be provected to a spirant (in Germanic initials) or lenited to a tenuis (in Indo-European), a tenuis may be lenited to a media (in all – the strongest tendency), and a media may be lenited to a susurrata (in Indo-European and later in Germanic as well – the term susurrata is used for a glide, murmur, voiced fricative, or other such continuous "whispered" weakly articulated obstruent in a series – see Griffen 1988:24).

2.2 Correspondences with Nostratic

This reconstruction of Germano-European consonants was conducted without any particular knowledge of the Nostrastic theory. Nonetheless, the Germano-European reconstruction fits far more precisely into the Nostratic reconstruction of Kaiser and Shevoroshkin (1988) and of Bomhard (1992 – also Bomhard and Kerns ms.) than do any of the Indo-European reconstructions. The Nostratic, Germano-European, and Indo-European reconstructions are summarized in table 2.

Table 2: Occlusive Obstruent Reconstructions

Nostratic	Germano-European	Indo-European
**p'	*p^h	*p
**p	*p	*b
**b	*b	*bh
**t'	*t^h	*t
**t	*t	*d
**d	*d	*dh
**k'	*k^h	*k
**k	*k	*g
**g	*g	*gh

As seen in table 2, the Germano-European and Nostratic reconstructions are practically identical in their occlusive obstruent series. The major difference is in the fortis-most obstruent, realized in Germano-European as an aspirata and in Nostratic as a glottalic. Since the glottalic feature involves a forced glottal ejection and the aspirate feature involves an emphatic glottal frication, that the former should give rise to the latter (or even vice versa) is not at all implausible.

The only other possible point of contention with the adherents of the Nostratic theory may be in the position of the media in Germano-European. Kaiser and Shevoroshkin (1988:323) consider the lenis-most realization of the obstruent in this family to be "breathy-voiced," to support borrowings from Semitic into Indo-European and from Indo-European into Kartvelian. Since the parts of the family in contact with these other families would have been the non-Germanic, postshift Indo-European groups though, this observation of borrowing patterns does not detract from the Germano-European hypothesis; nor does the Germano-European hypothesis detract from the Nostratic.

The obstruent system of Germano-European thus corresponds as precisely as could be expected with that of the Nostratic reconstruction. The phonetic basis in glottalic activity for the Nostratic reconstruction is quite compatible with the fortis aspiration of the Germano-European. Indeed, the same tactics may be expected to apply in both, at least to a significantly overlapping degree. Systematically, if the Nostratic theory were proven, then the hypothesis that Germano-European could be a member of that macrofamily would be very strong indeed – supported both by regular sound correspondences, which could come from borrowing or genetic relationship, and by the shared system, which could not be borrowed, but would be most likely a result of genetic relationship. Of course, there is the argument for a coincidence, but given the many possibilities for a phonetic basis of an obstruent system, this would be improbable; and it would be made even less probable by the extent of the contact areas between Germano-European and Nostratic.

3. The Altaic Obstruents

The primary source for this examination of the Altaic obstruent system is Poppe (1960:9-25, 42-62), drawn from many sources, the most important of which include Ramstedt 1957 and Vladimircov 1929 (compare also Shirokogoroff 1931). While there is considerable debate on the members of the family, the families that are consistently included in Altaic are the Mongolian, Manchu-Tungusan, and Turkic. Korean is increasingly included in Altaic (as Poppe does), and there has been some argument for Japanese (compare Miller 1971). Following Poppe (1960:8), the relationships among the Atlaic language groups can be visualized as in table 3 (p. 42).

The fortis-most obstruents of proto-Altaic are now consistently reconstructed not as tenues, but as aspiratae. Among Nostratic researchers, Kaiser and Shevoroshkin (1988) represent the fortes as markedly aspirate, and Poppe notes that "On the occlusives and affricates, it should be noted that they were differentiated not so much between voiceless and voiced as between strong (fortis) and weak (lenis). The strong

Table 3: Altaic Language Relationships

<table>
<tr><td colspan="5">"Greater" Altaic</td></tr>
<tr><td colspan="4">Altaic</td><td>proto-Korean</td></tr>
<tr><td colspan="2">pre-Turkic</td><td colspan="2">pre-Mongolian/Manchu/Tungusan</td><td></td></tr>
<tr><td>proto-Turkic</td><td>proto-Chuvashic</td><td>proto-Mongolian</td><td colspan="2">proto-Manchu-Tungusan</td><td></td></tr>
<tr><td>Turkic</td><td>Chuvashic</td><td>Mongolian</td><td>Manchu</td><td>Tungusan</td><td>Korean</td></tr>
</table>

occlusives (**p*, **t*, **q*, **k*) and the strong affricate (**č*) were probably strongly aspirated" (1960: – my translation from the German). While Poppe usually represents the fortis labial obstruent, for example, simply as **p*, the practice in this examination will be to represent it as p^h (or [p^h]), adjusting Poppe's transcriptions accordingly.

The following examination of the Altaic obstruents is conducted through the three degrees along the fortis-lenis scale – aspirata, tenuis, and media. All examples are taken from Poppe (1960) and are cited simply by page number in parentheses.

3.1 The Altaic Aspirata

Proto-Altaic maintained three plosive fortis obstruents – $*p^h$, $*t^h$, and $*k^h$. These all occurred in initial position and were evidently heavily aspirated, as seen in the changes they underwent in the development of the various Altaic languages. The affricate **č* was probably also fortis and may well have patterned itself after the other obstruents, but its development does not shed any light upon the nature of the Altaic obstruent system under study and is therefore not considered here in any detail.

The reflexes of the three Altaic plosive obstruents are listed in table 4. All of these occurrences are in initial position, which tends to be marked in an aspirate system for heavy aspiration (see section 2.1). The fact that the aspirata is found only here, of course, suggests what has traditionally been seen as allophonic variation and what is more precisely the coarticulation of an obstruction with an aspirate environment (compare Griffen 1985:chapter 5). This suggestion is addressed in section 4.

3.1.1 Labial. By far the fortis obstruent that is most supportive of the relationship with Germano-European is the labial. Through this, it is also most clear why the fortes are considered to have been heavily aspirated. As seen in table 4, the aspirated fortis $*p^h$ is realized in several ways in the Altaic languages.

In Manchu as well as in Monguor (Mongolian), the change from $*p^h$ to *f* is typical of provection (or fortition – a change toward the fortis) in an aspirate fortis-lenis system and parallels precisely the development in Germanic. As noted in Griffen 1988, such a change would necessarily have come from an aspirate obstruent in a fortis (initial) position. Thus, the correspondence between Goldic (Nanaj) *pāra* 'sled' and Manchu *fara* 'sled' (11) parallels that between Latin *pater*

Table 4: Altaic Aspirata

Labial - $*p^h$		Dental - $*t^h$		Dorsal - $*k^h$	
proto-Mongolian	$*p^h$	proto-Mongolian	$*t^h$	proto-Mongolian	$*q^{h3}$, $*k^{h4}$
Middle Mongolian	h	Written Mongolian	t	Written Mongolian	q^3, k^4
Monguor	f^1	Manchu	t	Tungusan	k, g^5
East Mongolian	null	Tungusan	t	pre-Turkic	$*q^3$, $*k^4$
Manchu	f	Turkic	t	Chuvashic	x^3, k^4
Goldic	p	SW Turkic	d^2	Old Turkish	q^3, k^4
Evenkic	h	Korean	t & th	Korean	k
pre-Turkic	*h				
proto-Turkic	*h				
SE & SW Turkic	h				
elsewhere	null				
Korean	p & ph				

1) in other positions *x*, etc. 2) and sporadically in other dialects 3) postvocalic 4) prevocalic 5) before *r*, *l*

'father' and Gothic *fadar* 'father'.

One could argue that the change from [p^h] to [f] could be a lessening of occlusion and therefore a case of lenition, as it is in the Irish mutation system, as opposed to a case of provection as it is in the Welsh mutation system. In Irish, however, the mutation only occurs in historically intervocalic, lenis environments – not in all initial positions. What is at work in Altaic is not a system based upon degree of occlusion, but one based upon aspiration.

Indeed, the realization of *h* in Middle Mongolian could also come only from an aspirate [p^h] undergoing provection, for example in the development of proto-Altaic *p^hokar 'short' into Middle Mongolian *hoqar* (11). This development also has its Germano-European parallel in Armenian, in which *hing* 'five' corresponds with Greek πέντε. If the starting point had been a tenuis [p], there would have been no source for the [h] either in Middle Mongolian or in Armenian (compare Brugmann 1972:357-58, Griffen 1988:193-94).

In the Turkic languages, the same aspiration occurred and remains in the southeast and southwest groups. In the rest, however, the labial obstruent has disappeared entirely, just as it has in eastern Mongolian languages as well – for example, the further development of Middle Mongolian *hoqar* 'short' as Mongolian *oqor* (11). The dialects with [h] show that the same process occurred in these as in Middle Mongolian and could only have come from a fortis aspirata. Moreover, the ultimate loss of occlusion has its parallel in the loss of the same obstruent in Celtic, leading to such correspondences as Old Irish *athir* 'father' and Latin *pater*.

As for Korean, the variation between tenuis and heavy aspirata would certainly be consistent with the Altaic system. Indeed, it would be consistent with an interpretation that Korean may be the most conservative, at least in its obstruent system (see section 4).

3.1.2 Dental. The development of the dental plosive obstruent *t^h is far less interesting than is that of the labial. Indeed, in all of the Altaic languages, the dental obstruents resisted change the most. Of course, change is not required in an aspirate fortis-lenis language – the patterns of change outlined in section 2.1 are simply the way in which one expects an obstruent system with such a phonetic basis to change, if change occurs.

The one exception to this pattern of conservatism occurs in the southwestern dialects of Turkic and sporadically in others as well. This is a change from *t* to *d* – apparently a case of lenition in a highly provective environment. Given the strongly provective pattern of change involving the labial and the maintenance of the fortis in the dorsal, this change is frankly puzzling and may reflect intermediate changes not attested in the data. While its extrasystemic behavior may mark it as an exception (rather than as a counterexample – compare Hjelmslev 1970:30-31) at least for the current investigation, its occurrence in one area and sporadic occurrence in others should warrant further study.

In Mongolian and Manchu-Tungusan, there was also a development from the aspirate dental to the affricate before the high front vowel, for example Mongolian *cida-* < *t^hïda-* 'to be able' (15). While one might be tempted to consider this a case of provection, as in the High German sound shift (see Griffen 1981b), the fact is that the same change occurred in all positions and was doubtless due to the simple retraction of the articulator in coarticulation with the vowel.

Once again, it is in Korean that the maintenance of the heavy aspirate is found, alongside the tenuis (or weak aspirata?). For example, proto-Altaic *$t^hatï$ 'to learn' yields Korean *that* 'domesticated' (13).

3.1.3 Dorsal. While it is customary to consider this position of obstruction dorso-velar or simply velar, it is quite evident from the alternation between *q* and *k* that the pertinent feature of the obstruction was its dorsality, with the precise point of contact determined to a great extent by the vowel or by the syllabic position (compare the German dorsal fricative in Griffen 1977, 1985:chapter 3).

The single development that stands out along the fortis-lenis scale – the realization of the plosive as a fricative [x] – is in the Chuvashic group. Actually, this development occurs in other Altaic languages in later stages, indicating a consistently aspirate fortis-lenis basis to the phonological system. For example *k^hak yields Mongolian *qag* 'dried dirt' but Khalkha-Mongolian *xagsū* 'hot, dry wind' (17); and *k^hut-*u-k* yields Mongolian *qutug* 'holiness' and Manchu *xuturi* 'holiness' (18).

This spirantization is totally in keeping with the provective pattern in initial position of an aspirate fortis-lenis system (the third expectation in section 2.1). It also parallels the development in Germanic, for example Gothic *hunds* < *$k^hu̯on$- 'dog' (compare Latin *canis*, Tocharian *ku*).

3.2 The Altaic Tenuis

In intervocalic position and in final and postconsonantal position, the fortis occlusives *p*, *t*, and *k* were realized as tenues. The implications for the aspirata in initial position and the tenuis in internal and final positions is treated in section 4.

The internal realizations of *p* follow two patterns – one in a "strong" position (before a short vowel) and one in a "weak" position (before a long vowel). These are represented separately in table 5 (p. 44) along with the other pertinent categories of internal and final realizations.

3.2.1 Labial. Once again, it is the labial position of obstruction that shows the greatest degree of change and provides the most striking evidence of the aspirate phonetic basis of the Altaic phonological system.

Mongolian exhibits clear evidence of an aspirate fortis-lenis system by following the first expectation of change as presented in section 2.1 and as realized in the non-Germanic (Indo-European) families in Germano-European. Thus, for example, proto-Altaic *t^hapa yields Mongolian *taba* 'satisfaction' (42), with the media *b* replacing the tenuis *p* in intervocalic position.

Manchu and Tungusan differ in these realizations despite their close relationship. Manchu exhibits a variety of

Table 5: Altaic Tenuis

Labial - *p

Intervocalic Strong		Intervocalic Weak		Final and Postconsonantal	
pre-Mongolian	*p	pre-Mongolian	*w	pre-Mongolian	*p
proto-Mongolian	*b	proto-Mongolian	*γ, *w, *j	proto-Mongolian	*b
Written Mongolian	b	Written Mongolian	γ, w, j	Written Mongolian	b
Manchu	f	Manchu	f	Manchu	-p-/-b
		Goldic	p/null		
Tungusan	p/w	Tungusan	p/w	Tungusan	p/w
Old Turkish	p	Old Turkish	p	Old Turkish	p
				Korean	-w-/-p

Dental - *t

Internal and Final	
Written Mongolian	d
Manchu	t
Old Turkish	t

Dorsal - *k

Final		Intervocalic	
proto-Mongolian	*q^1, *k^2	proto-Mongolian	*q^1, *k^2
Written Mongolian	g^1, g^2	Written Mongolian	q^1, k^2
Manchu	q^1, k^2	Manchu	q^1, k^2
Tungusan	k	Tungusan	k
Old Turkish	q^1, k^2	Old Turkish	q^1, k^2
		Korean	g

1) postvocalic 2) prevocalic

choices that may be indicative of a history of systematic change (but see below). The intervocalic *f* could result from gemination and its consequent provection in an aspirate system (compare Chuvashic *appa* from *äpä* 'older sister' – 43), or it could be indicative of a lessening of occlusion in a lenitive environment (compare Irish, section 3.1.1). In more expected and direct developments, the postconsonantal position does not change, and the final position weakens to a media *b*, indicative of an aspirate lenition.

The apparent confusion is cleared up by Tungusan, however, in its variation between *p* and *w*. The maintenance of the tenuis is quite simply no change – always a possibility in historical linguistics. The realization of the *w*, however, is a change to susurrata probably through the media *b* realized in weak position (see below). This is a pattern of lenition expected within an aspirate fortis-lenis system.

The realization of *f* in Manchu, then, would probably have proceeded through the *w/v* stage in a period of voicing. This is precisely what happened in Latin, leaving such correspondences as Sanskrit *bhrā́tar* 'brother' and Latin *frater*. (Compare Griffen 1982.)

In weak position (before a long vowel) the intervocalic labial tenuis is realized as in table 5, above. The variations in Mongolian are allophonic (that is, the position is determined by the coarticulated vowel) and are not of interest here, as the object of investigation is manner (degree along the fortis-lenis scale) rather than position.

Here the same types of patterns emerge, with the susurrata *w* (and its positional variants) realized in this highly lenitive environment in the Mongolian languages. Hence, proto-Altaic *$t^hapā$ 'to guess' is realized through the intermediate **tawa* as *taγa* (47). This is consistent with an aspirate fortis-lenis system, such as that in Germano-European. And once again, the realization of *f* in Manchu is not particularly bothersome, having a parallel in Latin, as shown above. Nor is the ever-present possibility that no change will occur at all.

Of particular interest, though, is the loss of occlusion in Goldic (Nanaj). Of course, the extreme lenition through a hypothetical media all the way to susurrata in Mongolian and Tungusan raises the possibility that this environment is so lenitive as to cause the ultimate lenition of the obstruent to null (as happens in final position in the Welsh labials and dentals – for example, the name *David* is realized as *Dafydd* [da:vɨð] and ultimately as *Dewi* [de:wi] – note also the further weakening in intervocalic position). In any case, the pattern of change is lenitive in an aspirate fortis-lenis system.

3.2.2 Dental. Aside from the affrication of the dental tenuis in Mongolian and Manchu, which is more of a positional change and not of consequence in this investigation (see section 3.1.2), the only change in manner to be found in either the strong position or the weak position is found in Mongolian, where the **t* is realized in Written Mongolian as *d*. For example, the name for a fire divinity **ōt+kan* is realized in Mongolian as *odgan* (49).

The intervocalic lenition of *t* to *d* is well represented in the Indo-European sound shift from Germano-European and yields such correspondences as Gothic *sitan* 'to sit' and Old Church Slavonic *sědĕti* 'to sit' or Latin *sedēre* 'to sit' from Germano-European **set-*. As such, it follows the most prevalent type of change within a fortis-lenis system – the first expectation in section 2.1.

3.2.3 Dorsal. The lenition of the internal and final dorsal tenuis is on average about as extensive as that of the dental, although far less extensive than that of the labial. In final position, Altaic **k* is realized in Mongolian as *g* or *q*, variants in the postvocalic and prevocalic position within the syllable. Thus, Altaic **cak* yields (Middle) Mongolian *čaġ* 'time', Khalkha-Mongolian *cag*, and Sagaic *sag* (54). In the other Altaic languages, the change did not occur, so that the word for 'time' in Old Turkish is *čaq* (54).

The realization of the media in final position is typical of the neutralization pattern inherent to an aspirate fortis-lenis language. For instance, the Welsh superlative form *tecach* [tekax] 'fairest' corresponds with the radical form *teg* [te:g] 'fair'. This may be opposed to a voicing language such as New High (East Central) German, in which the pattern is the opposite, with the plural *Tage* [ta:gə] 'days' corresponding with the singular *Tag* [ta:k] 'day'. (Compare Trubetzkoy 1969:76-77.)

As neutralization patterns in final position favor the unmarked (or less marked) member of an opposition, the realization of the media in this position in Mongolian clearly indicates a lenition along the fortis-lenis scale and supports an aspirate phonetic basis for the system. The lack of neutralization in the other Altaic languages does not necessarily indicate an absence of this basis – simply a nonoccurrence of the neutralization.

In intervocalic position, however, Mongolian usually resists the lenition of the dorsal, in spite of the fact that the labial and even the dental have undergone lenition. There are individual cases, however, in which the lenition does occur, and it occurs in some rather basic vocabulary items. For instance, the Altaic word for 'father' **etikè* is realized in Mongolian as *ečige* (56). Such realizations are in typically weak, lenitive environments, as in the third syllable (where accent falls on the first). The realization of lenition in a decisively lenitive environment – intervocalic and removed from the accent – is strong evidence for a fortis-lenis scale. Apparently, the tendency for such lenition is found in Mongolian, but it is so minor as to be realized in only the weakest environments.

The sole (apparently) Altaic language that maintains the lenition of the internal dorsal tenuis throughout is Korean. Hence, Lamutic (and Evenkic) *tẹ̀kūn* 'to become angry' corresponds with Korean *čigindā* 'to anger' (55). Indeed, this in addition to the heavy aspirate variant of the initial position (compare sections 3.1.1 and 3.1.2) strengthens the hypothesis that Korean may indeed be an Altaic language.

3.3 The Altaic Media

At least in initial position, the discriminative mark of the original Altaic media **b*, **d*, **g* may well have been the lack of aspiration (Poppe 1960:20). Thus, the only opposition that could occur in initial position – the opposition that is usually rendered as, for example **b*:**p* – would phonetically have been [p]:[p^h]. This opposition on the basis of aspiration without pertinent realization of voice would be typical of an aspirate fortis-lenis sytem, such as that of Welsh (compare Griffen 1985:chapter 7).

Unlike the aspirata and tenuis, which appear to have been to some extent positional variants (the aspirata in initial position, the tenuis in internal and final position), the media could appear in all positions. Since the discriminative mark of the opposition between the aspirata and the media was aspiration and that of the opposition between aspirata and tenuis was also aspiration, the differentiating opposition between tenuis and media must have also been in degree of aspiration. Thus, the aspirata would have been most aspirated, the tenuis less aspirated, and the media least aspirated (unaspirated). This three-way opposition of aspiration most clearly points to an aspirate system consistent at the outset with the aspirate fortis-lenis scale of Germano-European.

Once again, Korean deserves particular attention, for its obstruent system even today maintains three degrees of aspiration (compare Kim 1970). Assuming that it is an Altaic language, it would appear to be the most conservative of this language family. When one looks at the distribution of the Altaic language family and when one does add Korean to the family, then the geographic position of Korean on the extreme periphery and indeed on a peninsula blocking early language contact on three sides would lend this language the isolation in which conservativism might flourish (compare Bàrtoli 1925 – see section 4, below).

Maintaining the traditional media symbols *b*, *d*, *g* for this group, the development of the Altaic media in initial position and in internal and final position is outlined in table 6 (pp. 46, 47).

3.3.1 Labial. In initial position, all of the central group of Altaic languages maintain the media unchanged. Thus, the word for 'grey' is realized in Mongolian as *boro*, in Evenkic as *boroŋ*, and in Old Turkish as *boz* (20).

The only change occurs in Korean, in which the media is provected in initial position to a tenuis. For example, proto-Altaic **baragun* 'right' is realized in Mongolian *baraγun* 'right', in Goldic (Nanaj) *baro/baru* 'toward, in the direction to', and in Korean *parị* 'to be straight' and *paro* 'straight, direct' (21). Such a provection in this position is consistent with the third expectation for change outlined in section 2.1. While the change in this degree along the fortis-lenis scale is not typical in the Germano-European period, it is certainly found in the High German sound shift (compare Griffen 1981).

In final position, the labial media is subject to lenition to a susurrata in Mongolian and partially in Tungusan, quite in keeping with the development of an aspirate fortis-lenis language and parallel with changes in Germano-European –

Table 6: Altaic Media

Labial - *b

Initial		Final	
proto-Mongolian	*b	proto-Mongolian	*w[1], *b[2]
Written Mongolian	b	Written Mongolian	w/[1], b[2]
Manchu	b	Manchu	b
Tungusan	b	Tungusan	b/w
proto-Turkic	*b		
Old Turkish	b	Old Turkish	b
Korean	p		

Internal Strong		Internal Weak	
		pre-Mongolian	*w
proto-Mongolian	*b	proto-Mongolian	*γ, *w, *j
Written Mongolian	b	Written Mongolian	γ, w, j
Manchu	b	Manchu	b/w
		Goldic	null
Tungusan	w	Tungusan	w
Old Turkish	b[3]	Old Turkish	b

Dental - *d

Initial		Final and Internal	
pre-Mongolian	*d	pre-Mongolian	*d
proto-Mongolian	*d, *ʒ[4]	proto-Mongolian	*d
Written Mongolian	d, ʒ[4]	Written Mongolian	d, ʒ[4]
Manchu	d, ʒ[4]	Manchu	d, ʒ[4]
Goldic	d, ʒ[4]	Goldic	d, ʒ[4]
Tungusan	d	Tungusan	d
pre-Turkic	*d		
proto-Turkic	*j		
Old Turkish	j[5]	Old Turkish	d
		Evenkic, Lamutic	d
		Jakutic	t
		Sagaic	z

indeed, with the Indo-European sound shift itself. In Mongolian, the media is maintained at the end of monosyllabic verbal stems, but it is reduced to the susurrata *w (*u̯) elsewhere. Hence proto-Altaic *ab- 'to take' yields (Middle) Mongolian *ab-*, proto-Altaic **abuča* 'the taking' yields Mongolian *abuča* (44); but proto-Altaic **tabl(g)ai* 'have' yields (Middle) Mongolian *tau̯lai* or *tawlai* (44).

The difference between strong position (before a short vowel) and weak position (before a long vowel) affects the media as it does the tenuis (compare section 3.2). In the strong position, the Tungusan media undergoes a lenition much in keeping with an aspirate fortis-lenis system. Thus, proto-Altaic **téberi-* 'to embrace' yields (Middle) Mongolian *teberi-* and Lamutic *tẹwẹl-* with the same meaning (45).

More varied and interesting is the development in Turkic. While Old Turkish maintained the media, colloquial modern Turkic languages realize a *v* or a deletion of the consonant altogether – a clear weakening along the fortis-lenis scale. Moreover, in some cases, the labial media may be realized as the dorsal susurrata *γ* or *g̑*. For example, Old Turkish *äb* 'house' is realized in the Shoric and Küäric dialects as *üg̑* (45).

In the weak position, the Mongolian labial media fell together with the tenuis. This change must have originally involved the change from tenuis to media, for the realizations

Table 6 (continued): The Altaic Media

Dorsal - *g

Initial		Final	
pre-Mongolian	*g		
proto-Mongolian	*g̣^{6}, *g^{7}	proto-Mongolian	*g̣^{6}, *g^{7}
Written Mongolian	g̣^{6}, g^{7}	Written Mongolian	g̣^{6}, g^{7}
		Middle Mongolian	g̣^{6}, g^{7}
		Manchu	q^{6}, k^{7}
Tungusan	g, ŋ^{8}	Tungusan	k/g
proto-Turkic	q^{6}, k^{7}		
Old Turkish	q^{6}, k^{7}	Old Turkish	γ^{6}, g^{7}
Chuvashic	x^{6}, k^{7}		
Korean	k		

Intervocalic Strong		Intervocalic Weak	
proto-Mongolian	*g̣^{6}, *g^{7}	proto-Mongolian	*γ^{6}, *g̣^{7}
Written Mongolian	g̣^{6}, g^{7}	Written Mongolian	γ^{6}, g̣^{7}
		Middle Mongolian	null
Manchu	χ^{6}, x^{7}	Manchu	χ^{6}, x^{7}
Tungusan	g̣	Tungusan	g
Old Turkish	γ^{6}, g^{7}	Old Turkish	γ^{6}, g^{7}

1) final of nominal stems and internal 2) final of verbal stems 3) realized as susurrata in modern Turkic languages
4) before **i*, **ï* 5) realized in various Turkic languages as *ǯ*, *č*, *s*, etc. 6) postvocalic 7) prevocalic 8) before *n*, *l*, *r*

of both Altaic media and Altaic tenuis in this position all involve susurratae (see section 3.2.1). Thus, the Altaic word for a type of curds **abārtï* yields through intermediate **awārča* Mongolian *aγarča* (48).

In Manchu, the media may weaken to the susurrata *w*, but it may be lenited all the way to nonobstruction (null) in Goldic. In Tungusan, the lenition to *w* is more consistent.

3.3.2 Dental. The Altaic dental media has gone through some changes due to position, with variants of palatal obstructions realized in initial position of syllables with high front vowels (*i* or *ï*) in Mongolian and Manchu languages and in initial position of all syllables in Turkic languages. Only in Tungusan has the *d* been maintained in all positions. Thus, (Middle) Mongolian *del* 'mane' corresponds with Old Turkish *jil* (*jel*) 'mane', realized in Chuvashic as *śilxɛ* (22). Proto-Altaic **dïlugā* 'temple, bridle' is found in Mongolian as *ǯiluγa* due to the high front vowel, but it is realized as *ḏil* 'head' in Evenkic (23).

While there is no provection in initial position, as there is in the case of the labials, the lack of change is once again not particularly indicative of anything. Indeed, the palatalization may well have blocked any provection from the original dental media.

The same effects of palatalization can be seen in internal and final position. In Mongolian and Manchu languages, *ǯ* is realized before the high front vowels *i* and *ï*. Thus, while pre-Mongolian **edin* 'lord, ruler' is realized through *eǯin* as in (Middle) Mongolian *eǯen*, it is realized in Tungusan as Evenkic and Lamutic *ḙdī* 'husband' (53). In Turkic languages, it is realized in different ways, such that the word just cited is rendered in Old Turkish as *ädi* 'lord', in Abakan as *äzi*, and in Kasakhic as *ijä* (53).

The apparent provection of the media to tenuis in Jakutic is an isolated case and can be treated as exceptional. Nonetheless, this development may be related to the general resistence to lenition of the dental media among the Altaic languages.

3.3.3 Dorsal. Just as **k* has given rise to the variants *k* and *q* depending upon its position within the syllable, **g* is realized in Mongolian as *g* and *g̣*. Such variants are of no concern here, and in manner of articulation (degree along the fortis-lenis scale) Mongolian maintains the dorsal media in initial position.

In the Turkic languages, however, the dorsal media has changed to a tenuis, once again (as in the case of the dentals) following a pattern typical of an aspirate fortis-lenis

system. Provection in initial position creates such correspondences as Mongolian *gendün* 'masculine' and Old Turkish *käntü* 'self' (25). In Chuvashic, the variant occurring after the syllabic vowel (in final obstruction position) is realized as *x*, which may well be a further provection in initial position.

The nasalization that occurs in a Tungusan word when the following (syllable-final) consonant is *n*, *l*, or *r* is interesting, but it does not seem to affect the obstruent system in general. For example, the word for 'to fear' in Mongolian is *gelme-*, in Manchu *gele-*, and in Evenkic *ŋēlę* (25).

In Korean, word-initial **g* is realized consistenly as *k*, showing an initial provection in keeping with the Korean aspirate system. This means that only the two most aspirate, fortis members of the opposition can occur in initial position. This is somewhat reminiscent of the restriction prohibiting the lenis-most consonants from appearing in initial position in borrowings into Welsh (see Griffen 1975b).

In stem-final position, the media is maintained in Mongolian and Manchu (according to the data – the table in Poppe 1960:57 appears to be in error). Hence, proto-Altaic **bāg* is realized in Mongolian as *bag* 'section, group' and *bagča* 'bundle' (58). In Turkic, however, it is lenited to *γ* when it occurs after a vowel, as in the Old Turkish equivalent of the word cited *baγ* 'section of a tribe' (58).

In Tungusan, the media may be provected to a tenuis in a process of assimilation with the following consonant. For example, Evenkic *dęgī* 'bird' is related with *dęktę* 'feather' (58). Such assimilations are evidence neither for nor against the aspirate fortis-lenis nature of the system.

In intervocalic position, the dorsal media undergoes the same variation as does the labial. In the strong position (before a short vowel), the Turkic languages undergo an expected lenition to the dorsal susurrata *γ* when it is in stem-final position as well. Thus, for example **sógu* 'hart' is realized in Mongolian as *sogo* but in Solonic as *soγo* (58). From the data, a similar process appears to occur in Mongolian as well, yielding *aγu* from **agù* 'large, broad' (58). Other such lenitions to susurratae can be seen in such forms as Evenkic *aglān* and *awlān* 'steppe' (58).

The change from *g* to *x* in Manchu was probably achieved through an intermediate *γ* as well (compare Poppe 1960:61). Thus, for example, proto-Altaic **p*^{h}*ögü* 'nipple' is realized in Manchu as *fuxu*, probably through the intermediate **föγü* (61). As such, it would have been typical of the lenition to susurrata expected in an aspirate fortis-lenis system.

The same changes occur in weak position as in strong with one major exception – an exception that unambiguously points to the working of the aspirate fortis-lenis scale. In Mongolian, the media first changed to the susurrata *γ* in postvocalic stem position, maintaining the media in prevocalic stem position. By Middle Mongolian, however, the dorsal disappeared completely only to be realized as *γ* in modern colloquial Mongolian. The disappearance and reappearance is probably an illusion created by a shift of standard dialect – just as the change in dialect between Middle High (Upper) German and New High (East Central) German created the illusion that "German" changed such forms as *gân/gên* 'to go' to *gehen* (see Griffen 1983).

The interesting thing is that in those dialects typifying Middle Mongolian, the media in weak intervocalic position lenites first to the susurrata and then to total loss of occlusion. This pattern is seen in Welsh, particularly in the mutation system (compare Morris Jones 1913:164-65), and it parallels the pattern of change in German in which, for instance, Latin *magister* 'master' becomes German *Meister* (compare von Kienle 1969:111-12). The pattern in Mongolian is typified by the development (or alternation) from **bogàrla* 'to cut through the throat' to Middle Mongolian *bo'orla-* and Mongolian *boγorla-* (60). Such a development is clearly within an aspirate fortis-lenis system and parallel with developments in Germano-European.

Before the high front vowel, Mongolian is also further lenited to *j*. For example, the word 'healthy' developed from pre-Mongolian **sagīn* through intermediate **saγīn* to (Middle) Mongolian *sajin* (61). While this change is not precisely parallel to those in Germano-European, it is not inconsistent with them. Indeed, the same type of phenomenon can be found in Eastern German dialects in which *gesund* 'healthy' is realized with an initial glide.

4. Conclusion

From the foregoing investigation, it should be clear that the occlusive obstruents of the Altaic languages developed from proto-Altaic along an aspirate fortis-lenis scale. When changes occurred, they proceeded along the scale in keeping with the expectations of change within an aspirate fortis-lenis system. Although these changes were not always parallel with those in the development of the Germano-European languages, they were overwhelmingly consistent with them.

Moreover, the fact that the media contrasted only with the (heavy) aspirata in initial position and only with the tenuis in intermediate and final positions provides one of the most dramatic parallels in the relationship between Altaic and Germano-European. Without any consideration of Nostratic (much less of Altaic), the following conclusion was drawn for the ultimate occlusive obstruent system of Germano-European:

> On the basis of the evidence that can be evinced from the Indo-European languages vis-à-vis the Germanic, it is apparent that the first change to take place in the system was a slight provection in initial position as well as other positions of strength that may have resulted from shifts in stress. In these positions, the tenuis [t] became more heavily aspirated as the aspirata [t^h] (and likewise for the other positions). This yielded the three-member aspirate system [d], [t], [t^h], from lenis to fortis. (Griffen 1988:163-64)

The stage of development for proto-Altaic, then, is precisely the same as the stage of development for pre-Germano-European. It is no wonder that the two show such close parallels in their development, for they have proceeded

from the same kernel system along the same phonetic parameters.

Such a close systematic correspondence along with the multitude of lexical correspondences (see especially Krippes 1990b) would favor the hypothesis that Altaic and Germano-European derive from a common ancestor. Moreover, the developmental stage of the two-member aspirate opposition in proto-Altaic and the relatively recent development of the three-member aspirate opposition in proto-Germano-European would certainly point to the probability that the diversification of Altaic and Germano-European is fairly recent (compared with the ages represented by the proto-languages).

If in fact a genetic relationship exists (and the likelihood of that possibility seems quite strong given the agreement of the systematic and lexical evidence), then proto-Altaic would represent the more conservative branch – the branch that had not yet undergone the expansion of the two-member opposition to the three-member opposition. Indeed, an examination of the known development of Altaic should shed light on the hypothesized development of proto-Germano-European, for the development within the same phonetic parameter from the same base should yield similar patterns of change. Such is, after all, the precise phonetic nature of Sapir's concept of linguistic drift (Sapir 1921:chapter 7).

That being the case, the most conservative of all the languages in the Altaic and Germano-European family groups in their occlusive obstruent systems would appear to be Korean, if in fact it is an Altaic language (as may seem more likely in the light of this current investigation). In Korean, the three-member opposition of aspiration is most overtly aspirate in nature, as demonstrated by Kim (1970). And it more precisely follows the environmental strictures of the proto-language (although, to be sure, it does deviate from them in a way characteristic of an early departure from the protosystem).

Furthermore, given the areal linguistic considerations of Bàrtoli (1925 – see also Bolinger 1975:chapter 11 for English terms and a summary), Korean should be the most conservative from the norm of the isolated area and the norm of the later area. As pointed out in Griffen 1988:34-35, the remaining norm of the principal area is problematic anyway, and the norm of the lateral area would not apply. Should Korean prove to be an Altaic language, then, its conservative nature would be a consequence of its geography. As pointed out by Krippes (1990a), however, care should be taken in comparing Korean with other languages.

As for the greater development of Nostratic, no further conclusions can be made at this point. While the fact that both Altaic and Germano-European maintain an aspirata as the fortis could be taken to support Starostin (1989) in the contention that the Nostratic fortis was aspirate (a notion doubted by Shevoroshkin ms.), two families out of six is not enough at this point to tip the scales totally in favor of the aspirate, as opposed to the glottic.

On the other hand, the maintenance of an aspirate fortis in both Altaic and Germano-European may well indicate a close relationship between the two within Nostratic. Perhaps, these two families originally constituted a single dialect with the aspirata, as opposed to another dialect with the glottalic.

However, the ultimate composition of the Nostratic relationships depends upon considerably more study. The close relationship between Altaic and Germano-European at this point supports only that relationship. How these two tie in with the other four will depend upon subsequent investigations of the phonetic bases for those phonological systems.

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IE LARYNGEALS — EVER LISTENED TO THEM?

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Modern civilization almost is condemned to create the NEW. The new, all that differs from what hitherto has been, has no easy time though. Even experts sometimes are not capable to see a difference from the insignificant, the over-the-top, or the valueless. Is the fundamentally new possible at all? This is denied by F. Fukuyama and others claiming that postmodernists have witnessed the end of history, characterizing therefore the question of the new — that central category dating from the middle of the 19th century — as not opportune. Contrasting with this view, one can point to a constant dialectics of devaluation and revaluation, a turning of the valuable and the valueless, creating new oppositions and tensions without ever ending up in a synthesis. Nothing disappears totally but can always enter again the process of revaluation. Following the logic of innovation, the ground has to be dug constantly so that what is below comes up, and what is above goes down. Innovation, therefore, is displacement of things according to value boundaries.

The past decade has witnessed a revaluation of the genetic classification of languages and the implications of such a classification for the prehistory of the human species, thus digging up again 19th-century scholarship. As for the historical metalinguistics in the work of Joseph Greenberg, one surely would be able to set up another metalinguistics — or even let it be at all! *Mutatis mutandis*, Ludwig Wittgenstein seems to be relevant characterizing Sigmund Freud's theory of human prehistory as pure speculation, something even preceding the set-up of a hypothesis: Freud's metapsychology is indeed META-psychology.

Wittgenstein's countryman Ernst Mach (1838-1916) goes one step further: when being asked about the atoms, Mach simply answered: "Ever seen them?"

IN THE PUBLIC MEDIA

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SIBERIAN SITE CEDES STONE-AGE SURPRISE

On a windblown terrace above Siberia's Lena River, Russian scientists have unearthed evidence that humanity's evolutionary ancestors inhabited parts of northeastern Asia and could have made initial forays into North America much earlier than previously thought.

Preliminary soil analysis by two U.S. geologists

indicates that stone tools found at the Siberian location, known as Diring, date to around 500,000 years ago. However, Russian investigators date the artifacts to at least 2 million years ago, argued excavation director Yuri A. Mochanov last week in a talk at the Smithsonian Institution in Washington, D.C.

"I suspect the artifacts are younger than Mochanov's estimates," says Richard B. Potts, a Smithsonian archaeologist who examined a dozen stone flakes and blades brought from the site by Mochanov. "But even if Diring is only 50,000 years old, it's significantly older than any other human site in Siberia."

No other human sites in Siberia date to more than 35,000 years ago. This fuels the view that North America's initial settlers arrived no earlier than 20,000 years ago (SN: 6/9/90, p. 360).

Mochanov, an archaeologist at the Russian Academy of Sciences in Yakutsk, accepted this theory until shortly after he started working at Diring in 1982. Geologists digging up soil samples along the Lena River found some human bones and alerted Mochanov. He and his coworkers then excavated several human burials dating to 10,000 years ago and the 35,000-year-old remains of mammoth hunters.

The investigators also found sharp-edged stones that looked like human tools. These flakes, choppers, and other implements had been sandblasted by Siberian winds. Only East African stone tools that date to between 1.8 million and 2.5 million years old resemble the Diring artifacts, Mochanov contends. The tool-bearing soil has yielded no bones, probably because they were destroyed by windblown sand, Potts says.

A larger scientific team returned to Diring in 1983. Annual fieldwork since then has yielded more than 4,000 stone tools over an area the size of four football fields, making Diring the largest Stone-Age dig in the world, according to Robson Bonnichsen, an archaeologist at Oregon State University in Corvallis, who visited the site in 1992.

Measurements of magnetic reversals and radioactivity in Diring soil — the latter relying on a technique unknown to Western scientists — place the finds at 2 million to 3 million years old, Mochanov maintains.

He offers the radical proposal that direct human ancestors evolved not in Africa, but in the northernmost reaches of Siberia, where severe cold forced innovations in thought and behavior that fostered human evolution.

Archaeologists who have seen the Diring artifacts generally agree that someone intentionally made them, but they express skepticism about Mochanov's age estimates. In fact, thermoluminescence dates for two soil samples collected at Diring last summer by Michael Waters, a geologist at Texas A&M University in College Station, place the stone tools at about 500,000 years old.

Thermoluminescence dating of eight additional soil samples gathered by Waters will continue. Steven Forman of Ohio State University in Columbus directs the analysis, which estimates age from measures of the radioactive signal in sand grains and the dose of radioactivity in surrounding soil.

Ongoing soil and pollen analysis at Diring will help to establish whether its inhabitants endured bitter cold or lived during a relatively warm spell, Oregon State's Bonnichsen notes.

If cold weather prevailed, the Siberian findings will put a chill on the widespread opinion that only Neandertals adapted successfully to frozen climates, Potts asserts. Still, the species identity of Diring's inhabitants remains unknown.

Diring's estimated age of 500,000 years also supports theories that people could have migrated to North America more than 30,000 years ago, adds Smithsonian archaeologist Dennis Stanford.

Investigators should expand their Siberian search by launching excavations at 15 recently discovered sites located near Diring, Potts remarks.

— B. Bower

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ASIAN HOMINIDS MAKE A MUCH EARLIER ENTRANCE

Members of the human evolutionary family left Africa and reached eastern Asia 800,000 years earlier than previously thought, according to a report in the Feb. 25 *SCIENCE*.

The new estimate comes from a redating of three *Homo erectus* specimens from the Indonesian island of Java. Local collectors found a skullcap of a child at one site in 1936 and partial skulls of two individuals at another site in 1974. The first skull now dates to about 1.8 million years ago, the latter specimens to approximately 1.6 million years ago.

Carl C. Swisher III, a geochronologist at the Institute of Human Origins in Berkeley, Calif., and his colleagues analyzed the relative proportions of two forms of argon in the hominid-bearing sediment from the Indonesian sites to establish new dates for the finds.

Many anthropologists express surprise that *H. erectus* ventured to the far reaches of Asia so early. However, contrasting theories of how the *Homo* lineage evolved — which rely on analyses of skeletal anatomy — remain unchanged in the wake of the new study (SN: 6/20/92, p. 408).

"It shocks me that hominids [members of the human evolutionary family] lived outside Africa that early," asserts David W. Frayer of the University of Kansas at Lawrence. "We'll have to see if these dates hold up."

Frayer and others would prefer age estimates generated from sediment that still clings to the Indonesian bones, but Indonesian officials barred the removal of any material from the fossils, Swisher says. Comparable results at the two sites, located about 150 miles apart, compellingly support the new dates, he argues.

H. erectus apparently reached Asia before the appearance of stone choppers and hand axes in Africa around 1.4 million years ago, Swisher holds. This helps explain why no such artifacts have emerged from Asian sites, he maintains.

African fossils formerly assigned to *H. erectus* may belong to a separate species that led to modern humans,

Swisher suggests. In this scenario, championed by Bernard Wood of the University of Liverpool in England, *H. erectus* reached an evolutionary dead end in Asia.

G. Philip Rightmire of the State University of New York at Binghamton disagrees, based on his anatomical comparisons of Asian and African fossils.

"*Erectus* originated in Africa and then pushed out to Asia in pulses of movement," he argues. "The surprising new dates indicate that these migrations, and the *Homo* lineage itself, have more ancient roots than we thought."

Reasons for the migration of African *H. erectus*, often linked to the production of versatile hand-axes, now seem unclear, Rightmire contends. And much uncertainty surrounds the relationship of the *Homo* lineage to African australopithecines, which consist of the earliest hominid species. If *H. erectus* left Africa by 1.8 million years ago, its ancestor must have evolved simultaneously with various australopithecines, Rightmire notes.

Milford H. Wolpoff of the University of Michigan in Ann Arbor offers a third interpretation of the Java dates. Wolpoff lumps all *H. erectus* fossils into an anatomically diverse group of *H. sapiens* that evolved in several parts of the world starting about 2 million years ago.

"These new dates tell us that primitive *H. sapiens* left Africa much earlier than we thought," he holds.

Recent finds of simple stone tools at a Javanese *H. erectus* site that may date to 750,000 years ago suggest that Asian hominids probably concentrated on cutting bamboo with quickly produced implements, Wolpoff asserts; knowledge of hand axes may simply not have been put to use.

A related report, published in the March 3 *NATURE*, concludes that *H. erectus* and *H. sapiens* may have lived simultaneously in China for a short time.

Measures of the rate of uranium decay in animal teeth uncovered last year in the same deposit as a *H. sapiens* skull place the finds at a minimum of 200,000 years old, assert Chen Tiemei of Peking University in Beijing, China, and his coworkers. Some Chinese *H. erectus* remains date to 300,000 years old or less.

— B. Bower

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FOSSILS PUT NEW FACE ON LUCY'S SPECIES

Investigators have recovered and pieced together the first nearly complete skull of the earliest known species in the human evolutionary family, *Australopithecus afarensis*. Fragments of the 3-million-year-old skull, as well as of a number of limbs and jawbones, turned up at the Hadar site in Ethiopia. Fieldwork at Hadar in the 1970s yielded the first *A. afarensis* remains, including the partial female skeleton known as Lucy. William H. Kimbel and Donald C. Johanson, both anthropologists at the Institute of Human Origins in Berkeley, Calif., and Yoel Rak, an anatomist at Tel Aviv University describe the new finds in the March 31 *NATURE*.

"There is no obvious sign of evolution in this prehuman species for about 1 million years," Kimbel says. "Yet later, in only a fraction of that time, [*A. afarensis*] gave rise to a great branching of the family tree."

The age estimate for the new skull, assembled from more than 200 fragments found in a sandy gully in March 1992, makes it the youngest known example of Lucy's kind. Analysis of two forms of argon in crystals of volcanic rock just above and below where the skull lay established its age. The fossil's anatomy confirms that a 3.9-million-year-old cranial fragment previously found at another Ethiopian site also belonged to *A. afarensis*, the scientists argue.

Lucy herself lived about 3.2 million years ago.

Since 1990, annual fieldwork at Hadar conducted by Kimbel and his coworkers has yielded 53 *A. afarensis* specimens.

Kimbel attributes the Hadar skull to a male much larger than the diminutive Lucy, who stood about 3½ feet tall. The skull and other new Hadar material indicate that *A. afarensis* males were considerably larger than females, although average size differences between the sexes remain unclear, Kimbel notes.

A minority of researchers places smaller and larger *A. afarensis* in separate species. However, later australopithecines — including a lineage that died out 1 million years ago — also featured large size differences between sexes of the same species, writes Leslie C. Aiello, an anthropologist at University College in London in a comment accompanying the new report.

Reconstruction of the three-quarters complete Hadar cranium will yield an estimate of brain size, Kimbel notes.

A virtually complete ulna, or forearm bone, and a partial upper-arm bone found at Hadar also came from *A. afarensis*, the scientists say.

The ulna curves and is long relative to the upper arm, a pattern observed in chimpanzees. But the *A. afarensis* ulna lacks elbow features that allow chimps to support their body weight with the forelimbs while walking, Aiello says.

The thick upper-arm bone contains deep grooves where muscles attached, much like a corresponding *A. afarensis* fossil found at a nearby site (SN: 11/20/93 p. 324), Kimbel points out.

Aiello calls the arm bones "ideally suited to a creature which climbed in the trees but also walked on two legs when on the ground."

Some researchers argue that female *A. afarensis* favored tree climbing, as indicated by their curving toe bones.

Kimbel suspects debate about how Lucy's kind moved about will continue. *A. afarensis* clearly could walk efficiently on two legs, "but I don't know if we can say whether they spent time in the trees," he adds.

—B. Bower

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MODERN HUMANS LINKED TO SINGLE ORIGIN

A new study that calculates the mathematical fit of competing explanations of human evolution with the geographic array of specific fossil features supports a single African or southwest Asian origin for modern humans.

The analysis enters a heated debate over human origins. One theory posits an African genesis for modern humans between 100,000 and 200,000 years ago, after which *Homo sapiens* spread elsewhere and replaced Neandertals. An opposing view argues that modern humans evolved simultaneously in several parts of the world beginning about 1 million years ago, with genetic input from Neandertals (SN: 9/25/93, p. 196).

"Africa and southwest Asia are good candidates for areas where modern human anatomy originated ' asserts Diane M. Waddle, an anthropologist at Duke University Medical Center in Durham, N.C. "I'm confident that Neandertals had nothing significant to do with modern human evolution."

Waddle's study relies on a method, developed by geneticist Robert R. Sokal of the State University of New York at Stony Brook, for calculating the correspondence between various scientific predictions and sets of relevant data. Sokal and his coworkers have used this method to evaluate theories of modern language origins based on links between language patterns and genetic traits in European populations (SN: 8/22/92, p. 117).

The Duke scientist studied 83 fossil craniums of *H. sapiens* and Neandertals found at sites in Europe, southwest Asia, and Africa. Specimens ranged in age from around 40,000 to 400,000 years old. Waddle placed the fossils in 12 groups, depending on geographic location and age.

She then measured a series of cranial features and traits to calculate the anatomical variation in each group and the degree to which pairs of groups resembled one another.

Considered either as separate origin sites or lumped into a single group, African and southwest Asian fossils account better for the resulting pattern of anatomical relationships than the assumption that evolution took distinctive paths in Africa, Asia, and Europe, Waddle contends in the March 31 *Nature*.

Advocates of multiregional human evolution, such as Alan G. Thorne of Australian National University in Canberra, doubt that Waddle's conclusion will hold up once she studies East Asian and Australian fossils. Waddle plans to analyze these specimens, which have often been cited in defense of a separate Asian evolution of modern humans.

—B. Bower

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MAMMOTH TRUMPET REPORTS THE DEBATE ABOUT AMERIND DATES

HAROLD C. FLEMING
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Our friendly companion newsletter/journal, *Mammoth Trumpet*, had a particularly rich recent issue, focused primarily on the question of questions for American archeology. While the editor, Robson Bonnichsen, would let us reproduce the pictures and whole articles of that issue, it is beyond us. We've too much already. We do recommend once again that you-all subscribe to the other MT — *Mammoth Trumpet* (at Oregon State University). Or a large strumpet. We will skip the parts about the Siberian digs and hypotheses of the "mad Russian" (as some say), Professor Mochanov. Except to repeat what we reported earlier — he finds Oldowan tools in eastern Siberia at several my a and thinks humanity evolved in Siberia —, there is only one thing to add. Mochanov was worried about his dates, so he gave some of his material to an American lab which allowed that the stuff was maybe 500,000 years old, not several million. But if fairly early man lived in the ice box, and so near to Beringia, then that is a noteworthy discovery! Ah, but it depends on the tools! Can permafrost break up stones? When is a broken stone a broken stone and when is it Oldowan? (= from Olduvai Gorge).

That kind of question dominated the report on the great conference in Brazil on the validity and age of the Pedra Furada site. Was it really occupied more than 30,000 years ago by modern people? Were the admittedly crude stone tools really made by man? Among others, Scotty MacNeish, Tom Dillehay, Dina Dencause, and various professional skeptics and some European experts on stone were at the conference. They all could agree on very little.

The key questions for all the older-than-Clovis sites are: (1) How old are the various strata (layers of debris and earth)? (2) Is the site properly stratified, and have materials from one age intruded into the period of other materials? (3) Above all, are the stone, bone, or other materials made by man or are they products of natural processes? A geologist would care about God's stone work. We in human prehistory care about the stones only as evidence of what Bar-Yosef wants to call human behavior. One could excavate acres and acres of properly stratified stones and bones and shells and lord-knows-what — and get each stratum precisely dated and each object carefully plotted in relation to all the others. You could get a computer to arrange all this stuff in a kind of 3-dimensional view of the whole site so that you could see what things lay above or below or next to what other things.

But the whole bloody picture could be *meaningless* culturally!

As the *Mammoth Trumpet* report put it, the key question was: Are these stones geofacts or are they artifacts?

Did the action of common processes of nature (à la Lyell) produce these shaped stones? Or did the actions of humans produce them?

In Ethiopia, volcanic Ethiopia, one can find pieces of obsidian /balč'i/ almost anywhere one walks. One can pick up

a piece and drop it or throw it so as to break it. Since it is a lot like glass, one can quite by accident produce a razor-sharp piece of obsidian to use as a cutting tool. Nature can produce the same. So just a sharp edge does not necessarily imply an artifact. Unless there is some evidence of human work to get that edge or to "haft" it somehow, that sharp /balč'i/ is just a geofact. (To "haft" something means somehow to attach it to a handle or device for holding it).

Not only Guedon's site in northeastern Brazil but also Tom Dillehay's site in Chile (Monte Verde) at lower strata and also Scotty MacNeish's Pendejo Cave at lower strata — they each and all share the geofact versus artifact problem. The archeological skeptics are quoted as saying: Look, it's hard to tell the difference! If two of us cannot agree that this piece of stone is an artifact, then we are bound to conclude that it is either a geofact or too hard to tell, an unclassified piece of stone. Just a hunk of rock that happens to be in a stratum 30,000 years old!

OBITUARIES

HAROLD C. FLEMING
Gloucester, MA

All those who have escaped the Grim Reaper's wicked scythe can share the general sadness that we have to report. Three of our esteemed colleagues have departed for places unknown. We will miss them all, not only for themselves but also because their leaving diminishes us as a whole. Each was valuable. But also two of them were old friends of a quarter century or more.

MARIJA GIMBUTAS

Dr. Marija Gimbutas, author, archeologist, and one of our great experts on Indo-European prehistory, died from cancer on Wednesday, 2 February 1994, at U.C.L.A. Hospital in Los Angeles. She was 73 and had lived in Los Angeles.

Dr. Gimbutas was the author of 20 books and more than 200 articles on European prehistory and folklore. She was an authority on the Prehistoric incursions of Indo-European speaking people into Europe and how they changed society there.

Three of her more noteworthy books are *Goddesses and Gods of Old Europe* (1974), *The Language of the Goddess* (1989), and *The Civilization of the Goddess* (1991). Collectively, they present an interpretation of the neolithic period of Europe that challenged traditional views of prehistoric societies.

Perhaps her most controversial proposal was that the world was at peace during the Stone Age, when goddesses were worshipped and societies were centered on women. Then, the theory went, about 6,000 years ago, a European culture in which the two sexes lived in harmony was shattered by patriarchal invaders, and the worship of life-giving goddesses was replaced by reverence for war-like gods. Her studies and interdisciplinary approach also created a new field, archeomythology.

Although skepticism about her ideas was widespread among scholars, they were welcomed by many feminists and by Joseph Campbell, the mythologist. Writing about *The Language of the Goddess*, Gerda Lerner, a historian at the University of Wisconsin, said that although Dr. Gimbutas's theory could never be proved, it could "challenge, inspire, and fascinate" simply by providing an imaginative alternative to male-centered explanations.

She was the main proponent of the view that a warlike people speaking the language ancestral to all Indo-European languages conquered Europe and imposed their speech and culture on the conquered. A contrary theory, advanced by Colin Renfrew, holds that early farmers moving out of the Middle East across Europe transmitted a common language along with agricultural techniques.

A native of Vilnius, Lithuania, she received degrees from Vilnius University and a doctorate in archeology in 1946 from Tübingen University in Germany. She immigrated to the United States in 1949, conducted post-graduate research at Harvard University and in 1955 was elected research fellow of the Peabody Museum at Harvard.

Dr. Gimbutas joined the U.C.L.A. faculty in 1963 and served as professor of European archeology until her retirement four years ago. She directed five major archeological excavations in southeastern Europe.

Dr. Gimbutas is survived by three daughters, Zivile Gimbutas and Rasa Julie Thies, both of Los Angeles, and Danuta Lake of Anacortes, Washington.

(Adapted from the *New York Times*, 4 February 1994)

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SHERWIN J. FEINHANDLER

Sherwin Feinhandler died early this year at 58. He used to be amused by people who thought his label, Feinhandler, S. J., meant that he was a Jesuit priest and sought to confess to him. Others did on occasion confess to him because clinical psychology was one thing he did during some periods of his career.

Sherwin was a multi-faceted human being. The full roster of his professional activities can be read in the newsletter of the American Anthropological Association, written by John Mason and Tom Harford of Washington, D.C., plus myself. Suffice it to say for our purposes that he was at once an anthropologist, linguist, psychologist, businessman, teacher, and consultant. He would not be pinned down to one profession, nor one interest. He scorned specialization, although he was able to become awesomely well informed on a variety of topics (e.g., automobiles, computers, statistics, fine wines, drugs and their properties, etc.).

A graduate of Northwestern (BA) and Syracuse (MA), he got to Harvard just as the Chomskyite revolution was hitting the Boston area, especially in psychology. I can vouch for the excitement in the academic air those days. Riding the wave of

theoretical and interdisciplinary dynamics generated by that revolution, he gained a mental perspective on three fields which he never lost.

In anthropology he did his stint of field work, working with the Kamba of Kenya and among other things gaining fluency in Swahili. After his field notes were stolen, he roamed around East Africa for a while despairing of his doctorate. He ran a small business in the former Belgian Congo during the Lumumba period and watched UN operations with fascination, especially the cruelty of Ethiopian soldiers to the Congolese. Finally, regaining his morale, he returned to Harvard and got his Ph.D. But his lost field notes had cost him his credentials as Africanist and so he became officially a psychologist cum social science "type", not the Africanist he had wanted to be.

During his misfortunes in Africa, he observed that people often failed to treat him with proper respect but that upper class Englishmen lacked that particular problem. Coming from an upper bourgeois German Jewish background in Chicago, secure and treated well by the world, he had been shocked deeply by the great Holocaust, by the lingering albeit mild anti-Semitism of much of academia, and his precipitate drop in status in Africa. How he reacted to all this produced one spectacular theater of role-playing which lasted the rest of his life and became so habitual and embedded that his final self was the role.

Sherwin decided that he would become an upper class Englishman! This was preferable to being "a little Jew from Chicago", as he saw it. He worked on his RP (received pronunciation or aristocratic English) with his sharp linguist's ear; he married a beautiful young Englishwoman (blonde and blue of eye) and spent lots of time learning the culture of the British upper strata and then living that culture as his own. His efforts were doubtless helped by his good German Jewish genes. He was almost tall, lean and fit, blonde, blue-eyed and nearly as handsome as a movie star. Some were reminded of Ronald Coleman. And he was terribly smart; I've hardly met anyone brighter.

To reflect on his role-playing. What Sherwin managed to do was to hold the customary role (status) of an American social scientist, while playing it as if he were really Sir Laurence Olivier. He confused many people in academia and — such is the social insecurity of many people — he infuriated others who thought he was "putting on airs" or "talking down to them". After all, he was an American with a name like Feinhandler! And if he "really was only a little Jew from Chicago", how dare he speak with a British accent as if to raise himself above or belittle his listeners? And they punished him for it.

We need a bit of socio-linguistics. (*Mother Tongue* has obviously neglected that sub-field; it seems irrelevant to long range issues.) Foreigners may not know these details, just as Americans often fail to hear the nuances in the conversations of others. In the English-speaking world of America the dialect with the highest prestige is not a native dialect; it is England's RP. Americans look upwards socially to aristocratic Britain. With due respect to William Labov, whose fine work on American speech is saluted, he has missed this point. What shows up most in some old dialect areas around New York and Boston is imitation of RP of various periods. In 20th century America, a widespread form of this imitative RP is often called a "pseudo British accent". Perhaps the best example of this dialect is offered by the conservative William F. Buckley, Jr, who mastered an aristocratic drawl but not British vowels, or actresses like Lauren Bacall (contrasting markedly with Humphrey Bogart's more ordinary American). One can hear this Elegant English frequently on the East Coast, on campuses (English departments especially), in art circles, and among parvenus, the nouveau riche, etc.

Pseudo-British occurs internationally too. Some of us once heard in Mogadishu (Somalia) the voice of Bill Buckley coming from a Indian teaching English literature at Somali National University. An astounding replica of America's most famous pseudo-Brit!

Sherwin's great accomplishment was to master RP to the extent of getting it all but the localisms. Bill Buckley has fooled no Englishmen probably but Sherwin did. Several times I have heard Englishmen (in Boston pubs) ask Sherwin where he came from in England because they could not quite spot which exact local area it would be. He usually replied: "London." That great metropolitan place could sprout dialects as individual as his.

Since the clinician and participant predominated in him, we have few publications to attest to his theoretical prowess.

Sherwin became and remained for years a compleat Bostonian, Cambridge variety, but one with a British flavor. Most loyal of friends, good company and a powerful intellect. Oh, he is missed!

Postscript: Sherwin would be amused at the irony of his own case. For most of the past decade he ran and ran, until he developed into near marathon fitness. Most days he ran 10 km and he won a few races of that distance in his age group. He got so fit that his cholesterol level was normally under 160 and his body showed no fat. He even quit smoking. All this irritated the Grim Reaper so much s/he gave Sherwin leukemia just for spite.

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SUSAN PARK

Susan Park, formerly Mrs. Willard Z. Park, died of cancer at age 85 last October. Unfortunately, we were not informed until February this year because her family was severely grief-stricken and did not wish to accept her demise. A very sad state of affairs.

But as a consequence of that we lack much of the vital data on her earlier life. What we do know is that she came from a wealthy upper class family in San Francisco. They were German Jews in origin but seemed to have been spared the experiences with anti-Semitism that shaped the lives of others. Ethnicity played virtually no role in Susan's life — but politics did.

Susan studied anthropology at Yale in the 1930s, especially with Edward Sapir and George Peter Murdock. She also knew (as a peer) Morris Swadesh, whom she loathed. Her professional career as an anthropologist ended in the 1930s because she married another graduate student anthropologist,

Willard Z. Park. Being wife and mother consumed most of her time for the next few decades, while Willard became an important person in Latin American affairs for the U.S. government and, later on after the end of World War II, UNRRA representative in Ethiopia. Becoming deeply involved with reconstructing Ethiopia after the war, Susan and Willard fell in love with that country and stayed on by hook or by crook for the rest of their lives, or as long as they could.

Willard's career which had been flying at the ministerial level for some years was abruptly and brutally smashed by the House Un-American Activities Committee (U.S. Congress) in the 1950s. That unfortunate committee, a consequence of the Cold War, was not able to destroy the lives of everyone suspected of being a Communist; a few survived. Willard left the encounter no longer an employable anthropologist or bureaucrat; he was finished in those realms, even though, as he swore for years ever after, he had just been sympathetic to the plight of the downtrodden and had never been a member of the Communist party. Fellow Yale Man Morris Swadesh suffered the same fate, as is well known.

Picking up the pieces of their lives, the Parks moved back to Ethiopia and cobbled together an adjustment, partly export and import business and partly consultant work for government. Willard succumbed to a penicillin allergy in the 1960s. Susan eventually returned to the USA after the Ethiopian Revolution and lived in Nevada working part time on her Paiute ethnography and book. (She and Willard had first done "his" obligatory field work among the Paiute of the Great Basin [western USA].)

However, such a bare bones account of the facts of their lives neglects the considerable help they gave to the cause of anthropology in Ethiopia. Their home was a meeting place, their government contacts were numerous, their help unstinting. They saved my first field trip by helping me get a local job after my grant money ran out sooner than expected. They helped many others but also poked into many aspects of Ethiopia's numerous cultures as is the wont of their still beloved anthropology. What they did not do, sadly, was publish their observations, thoughts and conclusions. Susan continued poking around Ethiopia and talking with their very many Ethiopian friends for years after Willard died.

Having to do the marriage and family bit like a proper woman of her day, even though this cost her the doctorate in anthropology, Susan stayed faithful to the discipline that she traveled so far from her sheltered and privileged home in San Francisco to study. But the good she did for others was surpassing. It is still deeply appreciated.

QUICK NOTES AND HINTS OF THINGS TO COME

HAROLD C. FLEMING
Gloucester, MA

Quick Notes is designed to fill in the small holes of this issue. It is an awfully good way to present proposals in a nascent form (perhaps a fetal form would be more apt) and see what reactions they get. No one is responsible for bibliography or "proofs" or formal discussion in *Quick Notes*. Hunches reign here. It is also a good place to present small bits of news of interest to people. Let the italics present and define the topic.

Gérard Diffloth is back in touch, but just for a moment. Later in this spring, he is off to Cambodia for a year's sabbatical to search out the many small languages still undescribed and not yet assessed taxonomically. They are hiding in the jungles and other places. Some of that clearly will be dangerous to Gérard's health, but he says the mosquitoes are more to be feared than Pol Pot. Let us hope our good colleague brings back a Bolyu or Ongota or two!

Same fellow has to disagree, but amicably, about the classification of Bolyu. His hunch is that Bolyu (Lai) belongs in North Mon-Khmer near what used to be the Palaung-Wa cluster.

Same fellow has a "far out" hunch, nothing definite at the moment, that the Munda group of languages (in India) may be farther away from the rest of Austroasiatic than experts presently think. Munda presently is roughly the western half of Austroasiatic, or a sub-phylum. But maybe it is a proper phylum in its own right. For some reasons. Such a change would give India four native phyla (Burushaski, Nahali, Kusunda, Munda) plus its very old intruders (Dravidian, Tibeto-Burman) plus Indic.

John Colarusso has joined ASLIP, so as not to miss the uproar over his I-E is related to PNWC hypothesis. John will take the criticisms and respond vigorously but in his usual amiable way. John was been looking over Dene-Caucasic on his own and does think that Sino-Tibetan has a good chance of being related to Na-Dene (as Sapir, Shafer, and Pinnow thought). One wants to point out to long rangers that such an agreement is no small thing.

Kenneth Jacobs has joined ASLIP too, so our Canadian contingent is now quite robust! Ken is setting up a session at the American Anthropology Association meetings in Atlanta (late November) which will focus strongly on the prehistory of I-E. Three of our long rangers will be on the panel. Look forward to attending it!

Three of us in various discussions have concluded that there is one thing about the Chomskyite and post-Chomskyite period in linguistics that we dislike intensely. With all the emphasis on theory — nay, the tremendous over-emphasis on theory — most of the work of gathering the data on the whole human inventory of languages is being done by a minority of scholars. For some, like Diffloth, there is considerable personal danger involved. For others just hard work with precious little academic reward, because rewards seem to gravitate to theoretical hot shots. We field workers could use some help! Languages are dying out! Some of you need to back away from your computers and do *field work*! The experience will give you a "feel" for the languages you deal with, above and beyond

what you get from reading the publications of others.

Victor Mair has been written up in *Discover* magazine. I heard him also on National Public Radio (USA); he's been seen on TV. Those Sinkiang mummies are just so intrinsically interesting that Victor's fame is natural. As far as the I-E impact on early China is concerned, Victor can easily get into hot water with the modern Chinese. What Victor has said already may yet cost him dearly in terms of research permits — the Chinese officially at least do not wish to hear it. Such is an old problem in ethnography, as everyone knows.

Edwin Pulleyblank has been stressing I-E and Chinese connections for decades now. Maybe 30 years ago he discussed the Tocharians in this context and said essentially what Mair has now said. Victor in fact says that he hopes that everyone realizes that his report was not a formal publication because he did not list any bibliography. Had he done so we would have confronted a considerable literature on Tocharian and/or Iranians vis-à-vis China in a number of languages. Indeed nearly a half century before Pulleyblank's writings and 77 years before Victor's a German scholar had said very similar things. Plus ça change...

Tocharian or Iranian? A debate is shaping up about the language spoken by the Caucasoid mummies in Chinese Turkestan (Sinkiang). Good people on both sides. Good arguments on both sides. One says it was early Iranian or Indo-Iranian that was spoken in Sinkiang; the other says no, it was Tocharian. We really do not know, of course, but one can suggest that people be pressed to stipulate what reasons they have for choosing one alternative. Some of the reasoning so far has been rather sloppy.

Peer pressure. Just to show everyone how detached and scientific our heads are: consider this example. One colleague reports he is under considerable pressure from another colleague to "decide" that the mummies "must" have spoken Iranian. If someone asks him then, "why do you think the mummies spoke Iranian", he can reply: "because my colleague professor X obliged me to believe that!"

Another fossil companion for Lucy. The NY Times reported that new fossil finds in the Danakil (Afar lowlands) enlarged the range of characteristics of the Australopithecines, still 3.5-4.0 mya. (See p. 52 of this issue for the report in *Science News*.)

Ofer Bar-Yosef has much to tell us but no time to do it. He will give us a fuller report of his excellent activities in May when things cool down for him. We'll report on it in the Summer issue. We are pleased to report that Ofer stood for election to the Board of Directors of ASLIP (in absentia April 16th). He won.

Scotty MacNeish is a hard man to beat down. Now he has gone to an new site in New Mexico, there to continue his efforts to convince the skeptics. It probably would be easier to charm them or buy them off. Scotty will have the absolute world's record for digs!

The *Journal of Afro-Asiatic Languages* has risen like Phoenix from its supposed ashes, under the inspired direction of old friend, Ephraim Isaac (Princeton, NJ). This is deeply pleasing to many of us Afrasianists. A full field report on Ongota, sans taxonomy, will probably come out in two months. Rush your subscription orders to Ephraim Isaac, Institute of Semitic Studies, 195 Nassau Street, Princeton, NJ 08542, USA.

It is not too late to get on the programme for the *World Archeological Congress, Section 3, Language, Anthropology and Archeology* to be held in New Delhi, India, December 4-11, 1994. At this point, it's probably a good idea to send the title of your proposed paper plus a brief summary to Dr. Makkhan Lal, World Archeological Congress, P.O. Box 112, H.P.O., Aligarh - 202001, India. Phone (571) 29143, Fax (11) 6862049, Telex 564-230-AMU-IN. For electronic high-waymen, try Prof. V. N. Misra, Deccan College, Post Graduate & Research Institute, Pune - 411 006, India whose E.Mail address is dc ! mishra @ eucca. ernet. in. Their second announcement lists a fair number of our long rangers with, however, fairly low participation by Americans and Americanists.

It is also not too late for Africanists to plead to be included in the program of the *International Conference on Trends in the Historical Study of African Languages* at the University of Hamburg (Germany) on 5-7 September 1994. Interested parties should write to our very own Ekkehard Wolff or other organizers, Ludwig Gerhardt and H. Meyer-Bahlburg. Looks like some quite interesting papers by good solid people will be presented! Go!

For his many admirers we give the new address of our esteemed colleague *Jürgen Pinnow* (he has dropped the Hans). On his wee island in the North Sea, his address is: Gorch-Fock-Straße 26, D-25980 Westerland/Sylt, Germany. Write to him. He knows a lot!

Paul Benedict wants us to correct our misstatement of his belief about Bolyu. In MT-21, page 42, we say that Bolyu is "at best" a Mon-Khmer language. He thinks that makes no sense and wants long rangers to know that he said that Bolyu is "at heart" Mon-Khmer. On the question of Southeast Asia as a "nesting area", Paul thinks it was indeed for Austro-Thai, et al., and with "PST speakers on western fringe, early movements to south and west apart from Han to northeast. Movement from west unlikely. Best to consider native also — I see no good evidence for the tie with Caucasic et al. — but John (Bengtson) and I are supposed" to argue this point.

Same fellow accepts Hal's comparison of himself with Colarusso as much alike, although Austro-Tai is much more complex than his IE. But "I'll argue with him, however, about his view that what he calls 'Morphological cognates' are any more significant, or better maintained, than what he calls 'Conventional cognates'. In the Japanese/Austro-Tai book, I present over a dozen pages (123-136) on the former, here affixes of all sorts, as compared with over a hundred (161-264)

on the latter!" He has many things to say about reconstruction but we will save them for MT-23.

Andrew Merriwether of U/Pittsburgh, and an up-and-coming star in the biogenetics heaven, gave a talk on mtDNA in the Americas. He will publish much of it elsewhere but he has promised to give us a summary report in ordinary English soon. His study is the most comprehensive study of native Americans from the Arctic Eskimo to the Amerinds of Tierra del Fuego. He will give us a cladogram of quite a few "tribes" which we will match up against the Greenberg Amerind taxa viewed as a matching cladogram. It should be very interesting! One of his results, which is given here in a very preliminary form, is his conclusion that Eskimoans, Athapaskans and Amerind all go back to a single founding lineage before they join the rest of the world. I.e., his mtDNA findings do not confirm Torroni's findings or those of Turner and Segura which separate Eskimoan, Na-Dene, and Amerind peoples. At the moment, he seems to be saying that the three native American stocks are more like each other than they are like Asians but that the whole lot relates to eastern Asians (not necessarily Siberians) more than to the rest of humanity. Andy is very solid, and I urge people not to dismiss his findings out of pure frustration, not just yet anyway, until he has presented his full data and conclusions.

LETTERS TO THE EDITOR

7 March 1994

Dear Hal:

I enclose my check for membership in ASLIP. *Mother Tongue* is wonderful. I appreciate the copy you sent, and I enjoyed your comments on our paper a lot. I want to discuss some of the issues that your raised. I don't disagree with any of them, but a few points deserve more elaboration.

I am sorry that our paper seemed so technically complex. (I ought to point out that linguistics is forbidding to most of us and that there is an element of the pot's argument with the kettle here.) But in both of our fields, the technicality is necessary, and things will probably get worse rather than better in the future. You also remarked that we ignored mitochondrial "Eve", which is correct. I will write you a few paragraphs about both of these points, hoping to convince you that the genetic theory is not so bad after all, to explain why we ignore Eve, and to illustrate the perils of proceeding without the theory, under the assumption that data speak for themselves.

For three years after the papers about Eve appeared, I assumed that there was something that everyone but me understood and that my skepticism about the whole thing reflected some inadequacy on my part. Then I looked into it, talked with population geneticists, and realized that I was not alone in thinking that the estimated date of Eve was irrelevant to issues of modern human origins. Population genetics theory is gobbledygook not only to linguists but to many (most?) molecular geneticists, and the fuss about Eve was the result of doing genetics without genetic theory.

Here is a simple thought experiment that I use in my classes. There is an island with a population of exactly 100 humans on it, perhaps an experimental colony run by aliens. Every generation there are exactly 50 males and 50 females, and each female has exactly two children who survive. This island has been going for a very long time. So this is a population with no history, no dynamics, and no selection. Every single individual contributes an equal amount of genetic material to the next generation. Let us think about the mtDNA on this island.

Each woman has exactly two children each generation. About a quarter of the women have two daughters, a quarter have two sons, and half the women have one child of each sex. But we are concerned only with pedigrees of females through females when we study mtDNA, and this female-only pedigree is not so dull after all.

The fifty women on this island have roughly 37 mothers since the other 13 women last generation had two sons. So fifty women have 37 mothers. Going back another generation, our 50 women have about 27 mothers' mothers. Every generation the number of female ancestors through female-only lines decreases by about ¾. Before long, there is only one female at the root of the mtDNA tree, the mitochondrial Eve of this island population. Probability theory suggests that, on average, this island Eve lived 100 generations ago, say 2500 years assuming 25-year generations. (The expected depth of the tree in a population with N breeding females is 2N generations.)

What happened 2500 years ago? Absolutely nothing! This population has no dynamics, no history, no "origin", nothing at all.

So we get a grant to study mtDNA on the island, go through a mysterious computer analysis of the data, and derive an estimate that Eve lived 2500 years ago. What does this tell us? What it tells us (with a large standard error) is that the number of women on the island is 50. It doesn't tell us anything at all about origins or about history.

All of this is intuitive to population geneticists because, I suppose, they are trained to think this way. So what does the 200,000 year estimated root of our species' mtDNA tree suggest? It suggests that our species has 5000 breeding females each generation. (Mitochondrial DNA also suggests that there are 50,000 breeding female chimpanzees.) I have never been able to understand why the 200,000 year date was seen as support for the Garden of Eden model of human origins as opposed to the multiregional model. I think it is nearly irrelevant to the issue. As I said above, I assumed that I was missing something for three years, now I think that everyone else missed something!

A true fact from population genetics is that the speed of neutral change is inversely proportional to population size, so if a population shrinks it loses variability very fast because it is small. If it grows again to a large size, it recovers variability very slowly because it is big. Hence the estimate of 5,000 human breeding females is thought by many to reflect a rather severe bottleneck in our history. To be honest, very few of us would take this evidence from one locus very seriously if it were not concordant with evidence from other systems. The bottleneck in human history has been known about and widely

accepted by geneticists since the 1970s. What we tried to do in the *Current Anthropology* paper was to estimate when it occurred or, better, when the recovery began. If we believe current mutation rate estimates, it seems to have started around 50,000 years ago with a big standard error.

Here are a few other comments about your comments. It seems as if the ancestors of Africans were the first to undergo the late Pleistocene population growth, but *this does not mean that it happened in Africa*. Humans are very mobile, and I don't think we can assume that African ancestors were in Africa 50,000 years ago. Stan Ambrose and Richard Klein tell me that there is so far little or nothing in the way of archaeological remains from Africa between 75,000 and 25,000 years ago. (This is apparently something that all archaeologists know but none of the rest of us do.)

The available samples for our paper were far from ideal. They were a total sample of whatever was available scrounging in Mark Stoneking's lab at the time we wrote the paper. But I think that our results show that it does not matter very much for studying mtDNA. The reason is that most of the differences between individuals in the world are about 50,000 years or so deep (i.e. the peak of our waves). Most ethnic differences are much more shallow than this. In other words, mtDNA is good for looking at the difference between Europeans and Amerindians but almost worthless for looking at the difference between, say, Italians and Swedes.

I agree that it is time to look for the staging areas of the expansion of races. But remember that if these expansions occurred from populations on the order of 10,000 total size they could have been in pretty small restricted areas and hard to find today. One candidate worth considering is the 82,000 year old "Aurignacian" from the western rift in Zaire that Alison Brooks and John Yellen are writing up.

John Relethford and I have been doing some work that I want to mention to you because I want to throw the numbers we are getting at you and see what you think the consequences might have been for language evolution. We start with idea of rather isolated precursor populations in their respective staging areas and ask what the consequences would be for nuclear gene frequency diversity and the accumulation of differences. We then compare the theory with craniometric data collected by Bill Howells in the late 1960s that he has graciously made available to researchers. We chose three populations from each of four major regions in the Old World — Europe, sub-Saharan Africa, East Asia, and Australasia. The theory of all this is impenetrable gobbledygook that makes the other paper looks like child's play, and neither one of us really understands it all in any deep sense, but it is amenable to the cookbook approach. Our conclusions are (1) there was time for gene differences among races to equilibrate in 20,000 or so years, after which (2) population expansions would have "frozen" differences among groups. This means that many of the race differences we see today are quite easily accounted for by a model that says they accumulated 100,000 years ago because of population subdivision then.

The interesting conclusions are now (3) the ancestral African population was three times as large as the ancestral populations of Europeans, East Asians, or Australasians. That is to say the ratios of long term effective sizes of these regions were 3: 1: 1: 1, and (4) the rates of gene flow among regions were as shown in the figure below. This is taken from our submitted manuscript.

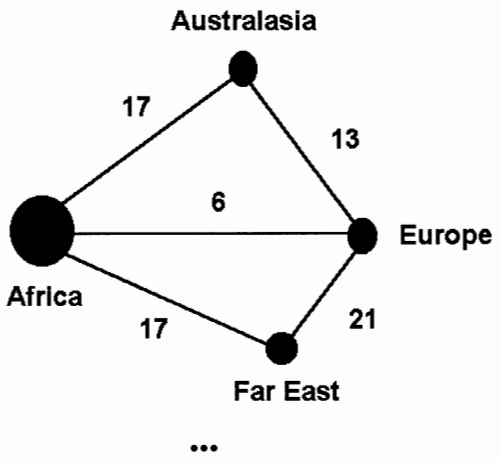
The numbers are the reconstructed number of migrants per thousand years. These are "effective migrants" so that old folks and kids would not count. These numbers are invariant under changing assumptions about how large the groups in the nesting areas were. In other words, craniometric variation implies that there were 17 migrants per millennium exchanged between ancestral Africans and East Asians no matter what the total population of African and East Asian ancestors was at the time. Note especially the implication that there was almost no gene flow between northern and southern Asians and that gene flow between African and European ancestors was mostly *via* Asian ancestors rather than direct. This pattern looks a lot like what Luca *et al.* come up with from other marker frequencies.

I am pretty enthusiastic about this work, but we need to repeat it on other traits like dermatoglyphics before we take it too seriously. We have no idea in the world what the standard errors of any of these numbers are. I expect that they are in the right ballpark regardless of details. What would happen to language here if one stray couple moved in every other generation or so? My guess is that it would have no effect on language differentiation in spite of its substantial effect on genetic differentiation. If this intuition is right, then language differences may be much older and deeper than genetic differences.

Finally, let me recommend the best summary paper about DNA and human evolution yet. Alan Rogers and Lynn Jorde wrote it — they have submitted it somewhere, but I don't know where. It is available through anonymous FTP from anthro.utah.edu in /pub. I believe that the title is "origins" but there are only a few papers there so it should be easy to spot. There is a postscript version (.ps), a TeX version (.dvi), and an HP version for direct printing to a laserjet (.hp).

Thanks again for the copy of *Mother Tongue*. Perhaps I will see you at the Africanist Archaeology meetings in Indiana?

Best regards,
Henry Harpending
Pennsylvania State University



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LETTER FROM YPSILANTI

Even being somewhat familiar with the harsh tone often used in MT contributions, we were still surprised by Hal Fleming's comments (MT 21, p. 68) on the Eastern Michigan workshop on "Nostratic: Evidence & Status". We organized the workshop intentionally as a small and informal working group, one convened because some historical linguists sought a scholarly dialogue about Nostratic between some practitioners and some skeptics. While no one's major questions were definitively answered, a number of us left with better appreciation for some other positions, a sense of what kind of evidence might confirm or disconfirm the hypothesis for us or others and so forth.

Following the workshop, a volume of papers, co-edited by Joseph & Salmons, was planned and is now in preparation, to explore further questions raised in Ypsilanti. We are aiming throughout to increase communication about some intensely held views, thus serving the interests of historical linguists of all ideological stripes, from the four members of your organization listed on the inside cover of MT whose work was presented (plus another who was invited but unable to attend and yet another whose work will be in the volume) to some very conservative Indo-Europeanists. This kind of interaction among people of differing views seems to us a normal part of scientific progress.

The ill-informed and insulting comments in MT only give credence to views like those expressed by William Poser (MT 21, p. 65) that a "cultish mentality...pervades...MT" and that "Discourse that relies on ad hominem attack and team spirit is unhealthy and unscientific." With him, we suggest that the focus be kept on presenting solid data and substantive argumentation, as we tried to do in Ypsilanti.

Joseph C. Salmons
Alexis Manaster Ramer
Brian Joseph

Organizing Committee,
Workshops on Comparative Linguistics

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RESPONSE TO THE LETTER FROM YPSILANTI

HAROLD C. FLEMING
Gloucester, MA

I ask the writers of the letter, complaining of my harsh words about their Nostratic conference, to accept my apologies. Truly! Sometimes I get carried away by the heat of combat with the forces of ignorance. Then I say inappropriate things. My remarks were inappropriate. Truly, I was responding to the dismay of some colleagues who were not invited. But I was wrong to criticize the conveners for having a small and select group of participants.

No doubt the writers in their irritation found it useful to retaliate by repeating Poser's remarks. But that is a non sequitur. Why do my inappropriate remarks show that Poser's remarks were right?

Everyone ought to remember why *Mother Tongue* exists in the first place and why its embattled efforts strike some others as "cultish". When was the last time you saw a cult that published all criticisms of itself? The real cult is the establishment in historical linguistics. They treat their beliefs as sacred dogma and do *not* allow free, open, and honest scientific discussion. Otherwise we would have argued, as Poser suggests we should, in journals like *Language* and *IJAL* long ago. You want us to stop speaking harshly? Fine, then get the editor of *Language* to let us publish on a fair basis, tit for tat, in her journal, instead of hiding behind "peer review" to keep us out, while allowing harsh words about us to flow from the likes of Rankin and Poser without any chance of our answering them. Let us all wait and see if *IJAL* has the scientific honesty to publish Greenberg's long rebuttal to his critics. Then we can all see if this dispute in the science of linguistics is a fair fight or not. Surely, no one is surprised to find paradigm defenders trying to control the flow of opinion or to squelch non-conformists. It is apparently famous in the history of science, although the innocents in the philosophy of science often seem not to know this. So we must say to our critics: don't you think there is just the faintest whiff of hypocrisy in the air?

ALL METHOD, NO CONTENT

HAROLD C. FLEMING
Gloucester, MA

Let this be the theme of Fleming's reply to the letter from William Poser, which appeared in the previous issue of *Mother Tongue.*

Having successfully resisted the impulse to "come out swinging with both fists", as they say in boxing, I decided that this space was an excellent place to discuss things with our opponents in a cooler way. Poser's letter (MT-21, p. 65) had a hard polemic edge to it as has most of his writing attacking Greenberg. It is awfully tempting to reply in kind. However, his letter and his review of Ruhlen's hook truly showed the themes of the opposition in a clear light. The full draft of the letter which he and several other colleagues sent to the *Scientific American* showed their position even more clearly. It ought to be published in *Mother Tongue* with ample space given for Greenberg to reply and others to comment as well. More than half of the original signers of that letter are also members of ASLIP. So here is a good opportunity to talk to them too. At least one of them is showing signs of becoming more reasonable.

Before going to the main theme, let me also respond to the vital accusation of not having read Poser's article in *IJAL.* Actually, he can say that only by distorting what I said about him in MT-19. It clearly did not depend on my having read his

article. Rather than making everyone go search for their copy of MT-17 (p. 53), I will give the full quote here:

> William Poser of Stanford mounts violent attacks on Greenberg's Amerind from time to time and apparently one of them got into *IJAL*. Hearsay. (We didn't get to *IJAL* yet this year.) But it would be splendid if one of our courageous Americanists actually left the womb and counter attacked once in a while. Naturally people would pass you in the hallways without speaking and no one would invite you to dinner anymore and life would be hell...

Indeed, I never read his article and probably never will either. What I was reporting was the news given by a colleague that Poser had attacked Greenberg in *IJAL*. That news was accurate and still is.

The main theme of Poser's MT letter and his review of Ruhlen's book is that Greenberg and Ruhlen are methodologically deficient and, therefore, their classifications, their taxonomies, cannot be right. Poser and his fellows have another theme, namely, that Greenberg's data are mistaken in case after case and so, therefore, his taxonomy cannot be right. His Iroquoian data are off, his Algonkian data are off, his Slouan data are off, his Salinan data (presumably) are off, they say. So how could he possibly be correct? However, we will not deal with these Americanist particulars here, mostly because Ruhlen or Greenberg himself in various places have answered them already. (For a Ruhlen response to Rankin's attack on Greenberg see *Annual Review of Anthropology*. Greenberg has written a long rebuttal to his critics, which has been submitted to *IJAL*.)

What is most striking in Poser's letter and his review of Ruhlen is the programmatic theme. Poser tells us how classification is to be done. (Rankin in fact does much the same.) Only after having avoided all the pitfalls of borrowing, influence, etc., and having lined up sound correspondences, can one begin to classify. If one cannot or will not do it that way, then one can be attacked by a solid mainstream linguist as being incompetent. (Poser calls Ruhlen "unreliable".) Besides this being a classic Indo-European statement of how one begins reconstruction, there is practically no successful classification in the real world (including Indo-European) that has been done this way. If our goal is not methodological conformity, it has to be taxonomy, n'est-ce pas?

Where, one might ask, is the Amerind taxonomy produced by Greenberg's opponents? They assure us that they are the experts on native American languages, not Greenberg. Well, then, where have they advanced the frontiers beyond obvious (easy) taxa (classes)? It is not a rhetorical question: tell me, where have they specifically done these things? Let the baseline of comparison be the taxonomy of Lyle Campbell's 1979 book on North America and Loukotka's 1968 classification of South America. So what do we find? What have they done? If we take Sapir's taxonomy as an earlier baseline, have we not gone backwards since his time?

At least for now, I have to conclude that a bunch ot idealists/programmers/perfectionists have told us that they must be correct, even though they have not yet produced any taxonomic advances, and that Greenberg must be wrong, even though he has produced a major part of the taxonomy of the entire world. Why would anyone want to abandon the schemes of a proven taxonomist, indeed one of the most gifted taxonomists to ever inhabit linguistics just to adopt the proposals of unproven methodological theorizing?

Where do these proposals come from anyway? Probably from refinements of Indo-European theory mixed with the overwhelming emphasis on rigor and analysis of 20th century linguistics. Why would anyone believe their proposals? Probably because nearly all teachers of historical linguistics tell them it is God's Own Truth and all righteous linguists should follow the tenets given them by their teachers. Clearly, Greenberg's most deplorable fault was not writing a popular introductory text in Historical Linguistics.

Maybe this sounds too sociological? Maybe it really is true that solid methodological theory is preferable to mere historical hypotheses? Perhaps, but there would still be a problem. Actually, two problems. First is that this standard or textbook theory is not well supported empirically. People just keep repeating it like the mantram it is and patting each other on the back for understanding it. Falsification would be its true destiny in a serious science, were it a true theory, but it is only what I have called "guideline" theorizing (cf. the discussion of dispersal theory in MT-21, p. 68). It adds up to a heuristic device — and quite a good one — for reconstructing ancestral languages. Read closely some of Henry Hoenigswald's statements on "the comparative method"; note that he *begins* with an existing taxonomy and proceeds to reconstruct the ancestor of the languages in the taxon. Note that he does not set up the etymologies either; nor does he judge their cogency. That is natural because he thinks mostly of Indo-European, an existing taxon where almost two centuries of discussion and debate have established the good etymologies and weeded out the bad.

Let one start out with Kikuyu, Masai, Luo, Boni, and El Molo — all of Kenya — and see how far he gets with these methods. It would be like using a microscope to find a boil on a nose. (Three very distinct phyla are found in this Kenyan group; Niger-Congo, Nilo-Saharan, Afroasiatic.)

The second problem in more general terms is the imposition of some social science theory on history. Two examples are given later.[1] I submit that what Poser and his colleagues are doing is imposing a scheme on Amerind data, a scheme which is correct ideologically and professionally but a scheme which basically prevents them from producing higher level taxa. Should we all hang by our ears, waiting for the vast Amazonian collection of languages to be reduced to even 10 or 20 genetic classes from its present 100+? Do not the opponents of Greenberg feel any pressure to get a move on? Are they not embarrassed that their New World lags far behind the rest of the world — taxonomically?

Not everyone knows it, but some in Poser's camp have been quietly searching for levers to pry loose the planks of Greenberg's African classification. What better way to refute Greenberg than to show that he was actually mistaken, and has been mistaken for 30 years? But alas, they have had no success. Perhaps it is helpful to repeat a few things that the critics cannot seem to remember.

ONE is that Africa has far more languages than the entire New World has, even including Eskimoan and Na-Dene — about 1475+ in Africa to 625+ in the Americas.

TWO is that Africa has much more phonetic diversity than the Americas or any other regions, even including the Caucasus.

THREE, perhaps because of its morphological conservatism, Africa has much typological[2] diversity but perhaps no more than, or even less than, the New World.

FOUR, in prominent or popular taxonomies of a few generations ago Africa had only two or three overarching "phyla"! These were the great typological schemes which gave typology such a bad name in Africanistics. Greenberg actually increased the African taxa to about 14 in 1953 before finally reducing the whole lot to four phyla in 1963! His were genetic classes, often with substantial typological dissimilarities within them.

FIVE is that very little of the ordering of the vast Africana was helped by reconstructions. A few existed in Bantu and Semitic. To put it much more forcibly — reconstruction, which is supposed to be necessary for classification beyond the obvious, was irrelevant to the general outcome. And that went far beyond the obvious. Indeed, some have argued that all Greenberg did was to pull together the obvious taxa and put them into four categories.

SIX is that the formal establishment of sound correspondences was basically irrelevant too. I say "formal" because the Greenberg brain was obviously seeing many informal correspondences and using them. But, despite his sophisticated brain, much of the classification did rest on plain old similarity.

SEVEN is that Greenberg sometimes used poor data. Sometimes the poor data, usually due to the fact that there was only one source on the language, impeded things a little. Some languages had to be put in their proper categories later on. But mostly the data were not so bad in general and perhaps as a whole not so much better nor worse than the Amerind data.

The EIGHTH and final thing is that, after thirty years of the 4-phyla scheme and forty years of the 14-phyla scheme, with lots of new languages being reported and eager young scholars trimming and pruning the branches — the African taxonomy is firm and strong.[3] It isn't going to be moved because it's the real thing!

It is natural for other anthropologists to see linguists as myopic and small-minded — and indeed, many of them really are. But we have the "my tribe", "my people" types in social anthropology who sometimes know virtually nothing except the incredibly interesting people they spent two years with. Or the archeologist who keeps returning to the same site year after year until he knows absolutely everything about very little. These are sometimes the folks they have teaching the introductory courses; luckily that work keeps them from becoming completely myopic. Yet all of us yield to the temptation of specializations, i.e., we think that our wee endeavors are the model of what others ought to do.

Some things about Poser's constant attacks on Greenberg and Ruhlen are deeply disturbing. His presumption is enormous. His preconceived ideas govern everything he writes about that I have seen. He dismisses Ruhlen's long hard work on the global set of human languages, and he scorns Greenberg's effort to bring a clear taxonomic picture to those hundreds of native American languages, even when Greenberg is still regarded by hundreds of social scientists as the unrivaled master of linguistic classification in our whole history!

One would think that Poser and his gang would be pleased at what Greenberg has done. One would think that they would be out there testing all the connections between groups and adding or subtracting etymologies — like scholars do in fields where people are civil to each other. One wonders whether Poser cares at all about taxonomy itself, the *content* of classification, and all the history that implies. For sure, he never seems to mention the specific content of Greenberg's Amerind hypothesis.[4] He reviewed Ruhlen's *Guide* without commenting on the classification itself — what the book was about! Only methodology, ever methodology, again methodology — yet his has serious flaws in it!

We will let Cavalli-Sforza handle Poser's attack on him, though he is more likely to ignore it. There is the matter of the last pious statement. I will disregard the comment about "cultish mentality" and that stuff. I find the last sentence rather amusing — now quoting:

> Only if they are able to present solid data and substantive arguments will proponents of distant genetic affiliation have an impact on the scientific community.

Would Poser know what that stuff was if it came up and kissed them? What do they think the Greenberg hypothesis is anyway? There is no solid data? There are no substantive arguments? Is it possible that Poser thinks that they alone represent the entire scientific community? Rest assured, long range hypotheses have already had an impact on the scientific community and will have more. What Poser and his gang should worry about is whether other scientists will begin to lose respect for the dog-in-the-manger style of linguistics which he represents so well.

AFTERTHOUGHT

There was a review of an interesting book on the history and philosophy of science. Written by John H. Brooke around 1987 and appearing in a journal whose name has been lost, the review makes some sharp comments on the subject dear to all of us. The editors are John A. Schuster and Richard R. Yeo. The full title is given because it will be hard to find this book: *The Politics and Rhetoric of Scientific Method: Historical Studies* (Australasian Studies in History and Philosphy of Science, 4), Dordrecht, Boston, and Lancaster: D. Reidel, 1986. (Distributed in North American by Kluwer Academic Publishers, Hingham, Mass.) Some quotes from the review:

> Do pronouncements on methodology, which scientists habitually make, lose their interest once it is appreciated that they rarely, if ever, determine scientific practice? If, as with Newton's famous but phoney "experimentum crucis", they have typically fulfilled rhetorical functions, should they not be disregarded? Emphatically not, is the message of this book; for the rhetorical and political functions of method discourse are precisely those that illuminate the social dimensions of science. This is because methodological prescriptions have assisted in the presentation of controversial theories, in the promotion of an individual's science as normative, in accounting for the erroneous character of rival science, in demarcation disputes between different disciplines, and in cultivating images of scientific unity likely to impress the public.
>
> The eight case studies presented here vindicate the editors' claim that a sociopolitical history of methodology can prove extremely fertile...
>
> However sterile formal methodologies may be, they have undoubtedly been appropriated for political ends ...
>
> The assumption that there was a single, accessible, and transferable scientific method was commonly displayed to encourage recruitment to scientific societies and to promote popular scientific education...
>
> Have methodological considerations never impinged directly on the content of scientific theory? Not all the contributors wish to go that far. Reporting the controversial launch of "continental drift", Homer Le Grand shows how Alfred Wegener adjusted the presentation of his case in response to methodological critiques ...

We will have more to say about Alfred Wegener in the future.

END NOTES

1) An example of this from a hostile British social anthropologist: He told me that Greenberg had amazing insight into the true forms of Hadza (Khoisan) words, not being fooled by the inadequate description in the source used. "Somehow he gets them right!", he said. Years later, at an international conference, he denied ever having said that and denied that Hadza was related to any other language in the world. However, two of us had heard his earlier remark, and I had recorded it in my notes.

1) One is from political sociology. A brilliant social theorist from Harvard proposed an acute sociological analysis of why Socialism never caught on in America. A historian reviewed the analysis and said the whole idea was brilliant but, unfortunately, false. Things had not happened that way in fact.

The second was a very neat sociological scheme (theory) for analyzing the evolution of Ethiopia as a polity, written by a friend of mine. As a reviewer, I had the unfortunate duty of pointing out that the scheme was invalid; things had not happened the way the scheme proposed.

These problems exist primarily because scholars impose explanatory theory on the narrative of history but without first establishing the narrative. This I derive from the wisdom of a great philosopher of science, Karl Hempel, who assumed that in the ordinary case the scientist would be dealing with an established narrative, like European military history or the dynasties of China. Then one cited the particular case (narrative) as an example of the operation of a general law — that explained it. While Hempel's theory is aging and needs repair, the distinction is useful to make. For example, one may want to explain the Russian, French, Ethiopian, and Mexican revolutions. Then one must obtain the narratives of each revolution but then one must have a general "Law of Revolutions" to explain each or any one. (The Russian and Ethiopian cases are fascinating in their similarity, both eliminated old ruling classes and both acquired even worse dictatorships.)

2) For non-linguists: We recommend that you treat the term "typological" as usually equivalent to "grammatical", meaning particularly morphology, syntax, and phonology. Two languages may be highly similar typologically but not have the same specific grammemes (bound forms usually) which can be shown to be cognate. Thus the languages may not show any evidence of genetic connection even when they are typologically similar. And vice versa. German and English are very close genetically but rather dissimilar typologically.

3) There is a fascinating parallel between the critics of Greenberg's African hypotheses and the critics of his American hypotheses. Once in Köln, I met a charming, bright, and well-spoken Hungarian linguist, named Istvan Fodor. Istvan had written a good-sized and rather impressive book attacking the African classification — root and branch. Istvan showed how Greenberg's methods could not possibly be correct, and he attempted to show how wrong in linguistic details Greenberg was, not overlooking the many data errors Greenberg had made. Istvan's book was designed simply to blow Greenberg and his whole approach right out of the water. Critics of the Amerind hypothesis should go look up Fodor's guide to African misclassification. But they should also note that Istvan Fodor had almost no impact at all on the final outcome. Perhaps a little in Hungary or maybe in Germany, because he was in Köln. *A good classification can withstand any number of methodological criticisms*. Why? This may sound corny, but probably a good classification is strong because it is *true*.

4) For example, some of our long rangers belong to the Cherokee nation. They can wonder about their roots just as much as an African American or a member of the "first families of Virginia" (FFV) can. They can get very little satisfaction from finding out that their roots go to Iroquoian and then maybe a little deeper to some connection with Siouan. But the traditional Americanist can only tell them that their roots are unknown, since properly prestigious scientific linguistics can only go back a few thousand years. Good thing our Cherokees have archeologists around to tell them they go back at least to the Clovis horizon or biogeneticists to tell them that their ancestors ultimately came from eastern Asia as long as 20 to 30 millennia ago.

ASLIP BUSINESS

Our annual meeting was held in Boston at the African Studies Center of Boston University on 16 April 1994. It was a great success in many respects, perhaps most of all: good fellowship.

The formal elections produced the following results (addresses are shown on the inside of the front cover of this newsletter):

President: *Harold C. Fleming*
Vice President: *Allan R. Bomhard*
Secretary: *Anne W. Beaman*

The President also serves as Treasurer. All correspondence about membership dues, orders for back issues, and the like should be addressed to him. (Some people have been uncertain about this.)

The following colleagues were formally elected to the Board of Directors (addresses are not given but can be obtained by inquiring to the President):

Ofer Bar-Yosef
Ronald Christensen
Frederick Gamst
John Hutchison
Mark Kaiser
Mary Ellen Lepionka
Philip Lieberman
Daniel McCall
Roger Wescott

Other Business of the Annual Meeting:

1) On the question of what to do concerning a member of the Council of Fellows because of his/her non-communication for very long periods of time and non-renewal of membership. The Board strongly disagreed with the President, believing that such a removal would serve no purpose. The Board rules ASLIP; hence an elected Fellow is elected for life, no matter what s/he does to or for us. The value of honoring Fellows is higher than any functional value a Fellow may have to ASLIP. We come to praise them, not use them.

2) On the date of the Annual Meeting: A member of ASLIP wished to have the By Laws changed so that the Annual Meeting could be held in March. That motion was voted down. In place of the existing wording, the following was substituted: "The Annual Meeting will be held on or about April 15th, preferably on the first Saturday after the 15th."

3) On the question of the nature of the Board and the Annual Meeting, the President proposed the following, which the Board and other members present did not challenge or disagree with:

a) The Board should be likened to a Board of Trustees, rather than to an executive group. Its primary duty is to preserve ASLIP and its values and purposes and to prevent either the officers or the editor of *Mother Tongue* from subverting those values and purposes. The Board can be likened to a gyroscope.

b) It is not necessary for the Directors to be linguists or physical anthropologists or archeologists or even to be intensely interested in the newsletter. Like the trustees of a university, they can be drawn from many careers. But their basic moral commitment to the good of the organization is paramount.

c) The Annual Meeting is primarily a meeting of the Board of Directors and the Officers, who perform their duties annually in conformity to the By Laws. Hence the Annual Meeting is largely a ceremonial or ritual occasion, although it can discuss anything new that it wants to and may dispute old decisions.

d) All power or final power in ASLIP resides in the membership. Were a large enough contingent of members to attend the Annual Meeting they could (theoretically) remove the current members of the Board and the current officers and choose ones more to their liking. That they have not done so is instructive.

e) Even though the time and place of the Annual Meeting have been clearly advertised in Mother Tongue for at least 5 years, no members from outside of New England have come to the meetings, until this year. Suggestions for annual meetings or occasional meetings at times and places other than the traditional ones have been met with indifference. The reasons for that are probably obvious to most of us.

f) Therefore, the President proposed that we draw the conclusion that we are primarily a group of readers, not conveners. Our older larger counterpart is *Current Anthropology*, rather than the AAA or LSA or SAA or ASPA. Whatever desires we may have had to be socially interactive are gone. Power lies in the membership, and the membership has clearly defined ASLIP as a publication.

g) So, though we may call some conferences at some points on some topics in trying to act out our basic charter, it will not be a regular or annual feature of ASLIP. We are essentially out of the convention business.

EDITORIAL

In this editorial, I would like to lay out some guidelines for the preparation of papers to be published in *Mother Tongue.*

General: *Mother Tongue* aims to appeal to a wide audience: linguists, anthropologists, archeologists, and geneticists, among others. Keeping this in mind, authors should avoid the use of technical jargon as much as possible. If it is necessary to use technical jargon, it should be carefully defined. Moreover, many of our readers are not native speakers of English. Therefore, authors should avoid using colloquial expressions. As a general rule, keep it simple, keep it clear, and keep it concise. All papers must be in English. Finally, try to keep the rhetoric civil. I can attest from personal experience, having myself been the target of ad hominem attacks in earlier issues of this newsletter, that it is not pleasant.

Cited Forms: Please try to avoid idiosyncratic symbols. If it is necessary to use special symbols, define them. For example, who, beyond a narrow group of specialists, would know that the symbol /ɣ̌/ represents a voiced alveolar lateral fricative, which is usually represented by the symbol /ɮ/? If in doubt about acceptable symbols, you may want to consult the *Phonetic Symbol Guide* by Geoffrey K. Pullum and William A. Ladusaw (Chicago, IL: University of Chicago Press, 1986) or *Patterns of Sounds* by Ian Maddieson (Cambridge: Cambridge University Press, 1984), both of which should be relatively easy to obtain. If you are trying to make a point and are using cited forms to illustrate your arguments, you will fail to convince if nobody can understand your examples because you have used idiosyncratic symbols. Or, refer to a standard (for example, Africanist, Indologist, etc.) so that others can translate your symbols into theirs. Best of all, create a table of equivalents to show just how your symbols match some known standard. Finally, *all* cited forms should be followed by an English gloss, thus: Egyptian *dd n-f nswt* 'the king speaks to him', Proto-Indo-European **medhi̯o-* 'middle', Monguor *ťšiärgi-* 'to swallow', Svan *mi* 'I'. Do not use 'id.'

Abbreviations: Beyond a few common abbreviations such as "mtDNA", "etc.", "et al.", for example, abbreviations should be avoided. This also applies to the names of languages and dialects, which should be spelled out in full. If you must use abbreviations, define them.

References: Bibliographical references should be as complete as possible. For format, we suggest that you follow the *Chicago Manual of Style* (14th edition, 1993), chapter 16 — note especially figures 16.1 and 16.2 on pages 647 and 648. Again, avoid abbreviations — spell out the titles of articles, books, and journals fully. Give page numbers. This does not apply to letters or *Quick Notes*, of course.

Allan R. Bomhard

MISCELLANEA

NEW BOOKS

The Origin of Language: Tracing the Evolution of the Mother Tongue. By Merritt Ruhlen. New York, NY: John Wiley & Sons, 1994, pp. xi, 239. $27.95.

"Just as archeologists study fossils and ancient artifacts for clues about mankind's origins, linguistic researchers today are sifting through word roots and grammatical conventions and coming up with startling revelations about our beginnings. In *The Origin of Language* noted linguist Merritt Ruhlen takes you on a fascinating journey of discovery back through nearly 100,000 years of human history and prehistory in pursuit of the language from which all modern tongues derive.

"Requiring no prior familiarity with linguistics, *The Origin of Language* is the first book to explain, in laymen's terms, the controversial process by which linguists are tracing the development of the vast range of human speech, sweeping aside many traditional assumptions about the spread of language and the roots of the human family. In addition to acquainting you with the manner in which such diverse languages as English and Chinese can be compared, Dr. Ruhlen introduces you to the brilliant mavericks whose linguistic theories are at last gaining worldwide acceptance. He also discusses the exciting new work being done in genetics and archeology that corroborates much of the controversial evidence.

"But more than simply describing his and his colleague's theories, Dr. Ruhlen invites you to share in the joys of discovery. He arms you with the linguist's basic toolkit and lets you work through the evidence for yourself and draw your own conclusions. You'll classify languages and language families, trace language family trees, and even reconstruct some of the basic vocabulary used by our most distant ancestors. Also, based on clues provided by your research, you'll plot the land and sea routes most likely taken by early humans in their diaspora out of Africa and to the four corners of the world.

"While the *Origin of Language* is an incomparable introduction to some of the most exciting linguistic research now being conducted by researchers around the globe, it is also much more. It is an inspiring invitation to join the quest for our human roots and to hear echoes of the Mother Tongue."

The Nostratic Macrofamily: A Study in Distant Linguistic Relationship. By Allan R. Bomhard and John C. Kerns. (Trends in Linguistics, Studies and Monographs 74.) Berlin and New York, NY: Mouton de Gruyter, 1994, pp. xi, 932. $199.00 (DM 298.00).

In this book, the authors present extensive evidence that certain languages/language families of Europe, Asia, and northern Africa are remotely genetically related. First, they agree with Greenberg that Indo-European belongs, along with Uralic-Yukaghir, Altaic (Mongolian, Manchu-Tungus, Chuvash-Turkic, Korean, and probably Japanese-Ryukyuan), Chukchi-Kamchatkan, Gilyak, and Eskimo-Aleut, to the Eurasiatic language family. Then, they maintain that Eurasiatic, as a group, is related to Elamo-Dravidian, Kartvelian, and, more distantly, Afroasiatic, all of which together constitute the Nostratic macrofamily.

Bomhard and Kerns begin by surveying the Nostratic languages. Next, they discuss the phonological systems of the Nostratic daughter languages, ending with a table of correspondences. Then, they trace the Indo-European phonological system through various stages of development. The following chapter is devoted to an overview of Nostratic morphology. Finally, the core of the book, running well over 500 pages, is the presentation of 601 Nostratic etymologies, supported by voluminous data from the various Nostratic daughter languages.

FONTS

Allan Bomhard has developed the *Phonetic Symbols Character Set* for use with MicroSoft *Windows* 3.1. (these are the phonetic symbols used in preparing *Mother Tongue*). The *Phonetic Symbols Character Set* contains over 800 fully scaleable True Type special phonetic symbols in the popular Times Roman typeface. Regular, *italic*, **bold**, and ***bold italic*** members are included. The phonetic symbols are spread across five font files, the first two of which contain vowel symbols, and the remaining three of which contain stop, fricative, and affricate symbols. Order follows that of the Latin alphabet. An IBM or compatible PC capable of running MicroSoft *Windows* 3.1 with at least 1.5 Mb of free hard disk space are required. These phonetic symbols are available for $20.00 (U.S. and Canada — $25.00 elsewhere) (the fonts are essentially being made available at no charge — the $20.00 is to cover the cost of postage, media, installation instructions, mailing envelope, etc.). Checks should be made payable to "Allan R. Bomhard". Orders may be placed with (please indicate preference: 3.5 inch or 5.25 inch diskette):

Allan R. Bomhard
73 Phillips Street
Boston, MA 02114 U.S.A.

MOTHER TONGUE

NEWSLETTER OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

AIM AND SCOPE

The Association for the Study of Language in Prehistory (ASLIP) is a nonprofit organization, incorporated under the laws of the Commonwealth of Massachusetts. Its purpose is to encourage and support the study of language in prehistory in all fields and by all means, including research on the early evolution of human language, supporting conferences, setting up a databank, and publishing a newsletter and/or journal to report these activities.

MEMBERSHIP AND SUBSCRIPTION INFORMATION

Annual dues for ASLIP membership and subscription to *Mother Tongue* are US $15.00 in all countries except those with currency problems (in those countries, annual dues are zero). Checks should be made payable to "ASLIP" and sent to:

Harold C. Fleming, *President*
Association for the Study of Language in Prehistory
16 Butman Avenue
Gloucester, MA 01930 U.S.A.

European distribution: All members living in Europe (up to the borders of Asia), and not having currency problems, should pay their annual dues to and will receive *Mother Tongue* from:

Prof. Dr. Ekkehard Wolff
Universität Hamburg
Seminar für Afrikanische Sprachen und Kulturen
Rothenbaumchaussee 67/69
20148 Hamburg
Federal Republic of GERMANY

Payment must accompany all orders (except as noted above). Selected back issues of *Mother Tongue* are available for US $4.00 each — for more information or to place an order, write Harold Fleming at the above address.